Popular Astronomy

Vol. LII, No. 4 **APRIL, 1944** **Whole No. 514**

Sunspots and the Weather

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SYNOPSIS

Both temperature and precipitation on earth depend on solar radiation. World temperatures have been rising since the American Civil War and are slated to continue rising for some decades to come. Temperature variations are greater in winter than in summer. Periods of extremely unseasonable cold are usually related to the dust thrown into the upper air from volcanic eruptions. Times of unseasonable warmth are usually related to sunspot minima.

Many close relationships have been found between the solar cycle and various terrestrial phenomena, such as (1) magnetic changes, (2) radio transmission, (3) the thickness of skin of fur-bearing animals, and (4) of tree-rings. Even industrial activity and stock market prices have been attributed to changes in solar activity. The proportion of ultraviolet light, closely related to vitamins and health, has often been related to the number of sunspots. Solar radiation has been believed to be the chief cause of major changes in climate and the downfall of ancient civilizations. With the warmth and northward shift of the stormbelt from 200 to 1000 A.D., the European balance of power shifted well northward to the Teutonic and Scandinavian peoples.

Many tropical pests, like termites and flying cockroaches, and tropical diseases, such as sleeping sickness, malaria, and dysentery, have been spreading northward in the unusual warmth since 1929. It may be that leprosy, yellow fever, and plague will again seriously menace people of temperate regions. Anticipating this, some of our medical colleges are specializing on tropical disorders.

The next year of sunspot minimum and warmth may be expected not later than 1946 and the next year of sunspot maximum and relative coolness would be expected about 1951. There are a number of practical effects which have already been indicated in agriculture and medicine from knowledge of the sunspot cycles. Weather forecasts are now made weeks and months in advance with some degree of accuracy. Such forecasts have been used commercially and probably also in the strategy of warfare.

In the magazine *Time* for January 17, 1938, the statement was made that Dr. Charles G. Abbot, head of the Smithsonian Institution, believes that both temperature and precipitation on earth depend on solar

radiation. This opinion by a distinguished observer of solar radiation at once commands attention. As regards temperature and the most dramatic form of solar radiation, sunspots, the following comment appeared in Dr. C. A. Mills' book "Medical Climatology" (1939). "World temperatures have shown a distinct rising trend since about 1860. Superimposed on this long-term rise in temperature level, which may go on for several centuries yet, we have sharp up-thrusts that come every few years, particularly during periods of diminishing sunspot activity." During the extended heat-wave of June, 1943, it occurred to me to examine some of the available figures to see whether they substantiate these two suggestions as to a relationship between earth's temperatures and solar radiation. Later I deal also with precipitation.

The sunspot numbers are available for each month from January, 1749, to the present time. For the period up to 1937 these are given in the book "Sunspots and Their Effects" by Dr. Harlan T. Stetson, and through additional sources the numbers have been obtained for some of the subsequent years. The mean monthly temperatures at Philadelphia, Pa., have been published for almost as long a period by the United States Weather Bureau. They start with the year 1758, and, with the omission of 27 years in the early period, continue to the present time. In passing, it may be noted that variations in the New York City temperatures were quite similar to those of Philadelphia. The temperatures in New York are usually about 2° F. lower.

The average number of sunspots during the entire period was about 46 per day; with a range from zero during each month of 1810 and at other times to 239 in May, 1778. The average temperature at Philadelphia was about 54°.4 F., while the monthly averages ranged from 22°.2 in February, 1934, to 81°.0 in July, 1793. Thus, while the average number of sunspots in a month was not very different from the average temperature in degrees Fahrenheit, the range in sunspot numbers was about 4 times as great as the range in mean monthly temperatures. Furthermore the temperatures normally followed a seasonal rhythm, being high in summer and low in winter, while the sunspot numbers ranged much more violently across the calendar and had maxima and minima in varied months throughout the year. During the period 1871 to 1910 the *daily* range of temperatures in Philadelphia was from a low of —6° F. in February, 1899, to a high of 103° F. in July, 1901; this makes a spread of 109° F.

In my brief note, "Monthly Sunspot Numbers," (POPULAR ASTRONOMY, November, 1942), I showed that the greatest number of maxima of sunspots occurred during the months which in the northern hemisphere are winter and the least number of maxima in the summer season. It is of interest to observe that the range of temperatures at Philadelphia (1871-1910) from lowest to highest reading was greatest during winter and least in summer. Table A shows this range in degrees Fahrenheit.

TABLE A

RANGE OF TEMPERATURES BY MONTHS
PHILADELPHIA, PA. (1871-1910)

Month	Range	Month	Range
January	77°F.	July	49°F.
February	81	August	50
March	81	September	62
April	75	October	57
May	62	November	69
June	52	December	75

During the summer months, April to September, the average range was 58°, twenty per cent less than that of the winter season, October to March, 73°. The smallest variations were in July and August (the hottest months); the largest being in February and March (among the coldest months).

There is naturally a much lower range in monthly *average* temperatures than in the corresponding daily readings, and of course the yearly averages are even more stable. If the deviations in the number of sunspots on a yearly basis are contrasted with the corresponding deviations in temperature averages there is no evident relationship. About half the time the sunspot deviation is of the same sign as the temperature deviation. Thus this crude comparison would give no indication of anything more than a chance relationship. It has been frequently noted as to weather in Philadelphia and New York City that it changes every four days as a general rule, with of course many exceptions and irregularities. Thus it seems possible that comparisons of sunspots and of temperatures over such a period as a week or four days might give some confirmation of the statement quoted above from Dr. Mills' book. The shortest period which I have available is a month, and even that relatively crude basis shows some relationship, as will now be indicated.

The monthly mean temperatures were compared with the long-time averages for the respective months. Notation was made of those months in which the deviation was more than one-tenth of the range shown in Table A. Thus deviations as large as +5° or —5° would be noted for June, July, and August, 6° for May, September, and October, 7° for November, and 8° for January, February, March, April, and December. The over-all average temperatures assumed for the respective months appear in Table B.

TABLE B

LONG-TIME AVERAGE MONTHLY TEMPERATURES
AT PHILADELPHIA, PA.

Month	Average	Month	Average
January	32°6F.	July	76°2F.
February	33.9	August	74.8
March	40.8	September	68.0
April	52.1	October	57.8
May	62.9	November	45.7
June	71.4	December	36.3

There were 27 months of excessively hot relative temperatures and

69 months of very cold relative temperatures. Of the former, 81% occurred after 1850 and 59% after 1870; of the cold months, on the other hand, 27% occurred after 1850 and 20% after 1870. Thus only about one-third as large a proportion of the cold spells as of hot periods took place during the years which Dr. Mills has characterized as a time of rising temperatures. The number of sunspots was tabulated for each of the months in the two lists. For the 27 "hot" months these averaged 31 and for the 69 "cold" months the sunspots averaged 55, which is 77% in excess of the average for the relatively hot months.

The 27 months of relatively excessive heat are shown in Table C with the number of sunspots, the average of which is 31 per day.

TABLE C

MONTHS OF EXCESSIVE RELATIVE HEAT AT PHILADELPHIA, PA. AND NUMBER OF SUNSPOTS

Month	Number of Sunspots	Month	Number of Sunspots
Feb. 1775	0	Jan. 1890	5
Aug. 1798	3	Mar. 1903	14
May 1822	2	Jan. 1913	2
May 1826	32	Mar. 1921	28
Feb. 1834	18	Dec. 1923	3
Feb. 1857	7	Feb. 1925	23
Jan. 1858	39	June 1925	48
Mar. 1865	40	Sept. 1930	32
June 1865	34	Sept. 1931	19
Jan. 1870	77	Nov. 1931	19
June 1870	136	Jan. 1932	12
Jan. 1880	24	Jan. 1933	12
May 1880	24	Jan. 1937	122
Sept. 1881	53		

It may be noted that two months have sunspot numbers quite out of line with the others in Table C. These are June, 1870, with 136 and January, 1937, with 122. Each of these occurred near the center of a period covering several months of numerous sunspots. These therefore seem to be distinct exceptions to the assumed hypothesis that "sharp upthrusts (of heat) come during periods of diminishing sunspot activity."

The 69 months of relatively excessive cold are shown in Table D with the number of sunspots, the average of which is 55 per day.

TABLE D

MONTHS OF EXCESSIVE RELATIVE COLD AT PHILADELPHIA, PA. AND NUMBER OF SUNSPOTS

Month	Number of Sunspots	Month	Number of Sunspots	Month	Number of Sunspots
May 1759	49	Oct. 1777	106	Dec. 1815	20
Aug. 1759	71	June 1798	0	June 1816	44
Aug. 1768	67	Apr. 1801	31	July 1816	39
Oct. 1768	78	Aug. 1806	26	Aug. 1816	23
Nov. 1769	148	Dec. 1809	0	Sept. 1816	48
July 1771	68	Aug. 1810	0	Dec. 1819	31
Mar. 1772	31	Dec. 1810	0	Dec. 1820	10
May 1772	38	Dec. 1812	10	Dec. 1821	0
July 1777	95	Dec. 1813	14	Aug. 1827	46
Sept. 1777	116	Feb. 1815	32	Feb. 1829	49

Month	Number of Sunspots	Month	Number of Sunspots	Month	Number of Sunspots
Sept. 1829	53	Jan. 1840	81	Dec. 1872	84
June 1831	33	Sept. 1840	74	Nov. 1873	55
Dec. 1831	29	Oct. 1841	28	Apr. 1874	32
Feb. 1836	108	Nov. 1842	40	Feb. 1875	22
June 1836	125	Mar. 1843	8	Dec. 1876	8
Aug. 1836	108	Dec. 1845	60	Feb. 1885	72
Oct. 1836	137	Feb. 1848	112	Mar. 1885	50
Dec. 1836	206	Jan. 1856	0	Oct. 1888	2
Aug. 1837	134	Mar. 1856	0	Jan. 1893	75
Feb. 1838	85	Jan. 1857	14	Feb. 1895	67
Oct. 1838	91	Mar. 1859	90	June 1903	16
Dec. 1838	80	Feb. 1868	16	Dec. 1917	129
June 1839	55	Mar. 1872	88	Feb. 1934	8

In Table C there were five years each of which was represented by two months of abnormally *high* temperatures. These were 1865, 1870, 1880, 1925, and 1931, the last of which was upon the whole the warmest year in the entire series from 1758 onward.

In Table D, on the other hand, there were fifteen years in each of which there were two or more months of very *low* relative temperatures. One of these, 1836, was represented by five months, in which the number of sunspots was very large, reaching its culmination in December with 206 sunspots. This was the second coldest year in the entire series. The coldest year, 1816, was represented by four successive months of extreme cold, June, July, August, and September. This has long been famed as "the year without a summer." Not only the year but the months of June and July also were the coldest of which we have record. There were two years represented by three months each, namely, 1777 and 1838. The years with two months of excessive cold were 1759, 1768, 1772, 1810, 1815, 1829, 1831, 1840, 1856, 1872, and 1885. It will be observed that these are all rather early in the series as compared with those from Table C as listed in the preceding paragraph.

How great an influence on the weather can be exerted by a few degrees variation in *average* temperature for a month may be seen from the conditions in June and July, 1816. The average temperature for June (64°.0) was only 7°.4 below normal and the average for July (68°.0) was only 8°.2 below the normal for that month. Yet in each of these months there were on a number of days heavy frosts and ice as thick as a windowpane and nearly everything green was destroyed.

A similar review of precipitation at Philadelphia, Pa., reveals less relationship to the number of sunspots. There were also wide differences between the rainfall in Philadelphia and that of New York City. The earth retains heat for some time and thus readings of temperature are more stable than those of precipitation. The heat-retaining power of the earth may be seen in the fact that the coldest winter days usually occur a month or six weeks after the shortest day. Similarly the hottest days are usually characterized by a like time-lag after the longest day

in the year. The study of precipitation does, however, have points of interest and the material may be useful to other investigators.

The publications of the United States Weather Bureau give the precipitation at Philadelphia, Pa., for each month beginning with January, 1820, and I have carried the series over a period of 120 years. The maximum and minimum precipitation for the respective months was noted for the years 1820-1910. These appear in Table E.

TABLE E

RANGE OF PRECIPITATION BY MONTHS AT PHILADELPHIA, PA. (1820-1910)—INCHES

Month	Maximum	Minimum	Range
January	7.84	.48	7.36
February	6.87	.55	6.32
March	6.98	.38	6.60
April	9.76	.59	9.17
May	9.46	.19	9.27
June	11.03	.74	10.29
July	11.80	.75	11.05
August	15.82	.41	15.41
September	12.09	.20	11.89
October	10.05	.30	9.75
November	7.31	.67	6.64
December	7.37	.26	7.11
Year	61.20	29.67	31.53

The normal amounts of precipitation assumed for the respective months are given in Table F; the corresponding figure for the year being 41.51 inches.

TABLE F

LONG-TIME AVERAGE MONTHLY PRECIPITATION AT PHILADELPHIA, PA.

Month	Average (Inches)	Month	Average (Inches)
January	3.26	July	4.08
February	3.34	August	4.71
March	3.35	September	3.36
April	2.99	October	3.01
May	3.27	November	3.12
June	3.32	December	3.06

Tabulation was made of the months during the 120 year period in which the deviation of precipitation from normal (Table F) exceeded three-tenths of the range (Table E). Where the lower limit so obtained was less than the minimum, it was adjusted to twice the minimum for the month. This procedure gave 135 months of excessive relative wetness and 69 of excessive dryness. Of the former, 58% occurred prior to 1880 and of the dry months, only 43%. The average number of sunspots in the wet months was 45 and in the dry months, 41. When these results are compared with those for temperature, it may be observed that "wet" corresponds to "cold" and "dry" corresponds to "hot." Thus while the temperature has been increasing the precipitation has been decreasing. There was not, however, the same sharp differentiation for snow and rain as for temperature. In Table G are shown the

69 months of excessive relative dryness and their number of sunspots, the average of which is 41 per day.

TABLE G

MONTHS OF EXCESSIVE RELATIVE DRYNESS AT PHILADELPHIA, PA. AND NUMBER OF SUNSPOTS

Month	Number of Sunspots	Month	Number of Sunspots	Month	Number of Sunspots
Jan. 1821	22	Feb. 1872	120	Apr. 1896	44
Mar. 1821	6	June 1873	45	Aug. 1896	27
Aug. 1821	5	Feb. 1877	9	Apr. 1899	14
June 1823	0	Aug. 1877	6	June 1899	20
Jan. 1825	5	Dec. 1877	2	Feb. 1901	2
Apr. 1825	4	Feb. 1879	1	June 1901	6
May 1826	32	Oct. 1879	12	Nov. 1903	44
Mar. 1827	58	Apr. 1881	52	Sept. 1906	56
Dec. 1828	47	July 1881	45	Nov. 1908	46
Feb. 1833	15	July 1882	45	Mar. 1910	21
Apr. 1833	3	Nov. 1882	84	Mar. 1915	39
Aug. 1834	4	Sept. 1884	62	Nov. 1915	42
Feb. 1841	30	Mar. 1885	50	Nov. 1917	96
Sept. 1846	109	June 1885	84	Oct. 1921	16
Apr. 1847	45	June 1888	7	Nov. 1922	7
Jan. 1849	157	Dec. 1889	7	June 1923	9
June 1853	40	June 1890	1	Oct. 1924	26
Feb. 1856	5	Nov. 1890	10	June 1925	48
Feb. 1857	7	Feb. 1892	76	Mar. 1927	70
May 1858	41	Oct. 1892	70	Nov. 1931	19
July 1858	57	July 1894	106	Nov. 1933	1
July 1860	117	Feb. 1895	67	Nov. 1936	113
Feb. 1864	47	Aug. 1895	68	July 1937	145

The 135 months of relatively excessive wetness are shown in Table H with the number of sunspots, the average of which is 45 per day.

TABLE H

MONTHS OF EXCESSIVE RELATIVE WETNESS AT PHILADELPHIA, PA. AND NUMBER OF SUNSPOTS

Month	Number of Sunspots	Month	Number of Sunspots	Month	Number of Sunspots
May 1820	29	Dec. 1841	39	Nov. 1860	98
Oct. 1820	9	July 1842	13	May 1861	57
Mar. 1823	1	Nov. 1846	60	June 1862	84
Dec. 1823	20	Sept. 1847	161	Mar. 1863	66
July 1824	0	Dec. 1847	110	Apr. 1863	41
Mar. 1826	37	Mar. 1849	96	May 1864	41
Oct. 1827	57	Dec. 1849	97	Sept. 1864	28
Nov. 1828	57	May 1850	62	Feb. 1865	39
Nov. 1830	81	Sept. 1850	86	May 1865	35
Jan. 1831	48	Apr. 1852	65	Sept. 1865	22
Oct. 1833	8	Nov. 1852	54	Dec. 1865	13
Dec. 1833	10	Apr. 1854	26	Feb. 1866	38
Jan. 1836	89	May 1854	24	Sept. 1866	7
June 1836	117	June 1855	5	May 1867	3
June 1838	94	Dec. 1855	3	June 1867	2
Sept. 1838	74	Apr. 1857	11	Aug. 1867	5
May 1839	54	June 1857	16	May 1868	27
Dec. 1839	64	Dec. 1857	37	Sept. 1868	44
Apr. 1840	66	Nov. 1858	52	Nov. 1868	59
Jan. 1841	24	Jan. 1859	84	Oct. 1869	59
Mar. 1841	30	Mar. 1859	90	May 1870	176
Apr. 1841	43	Sept. 1859	106	Mar. 1871	143

Month	Number of Sunspots	Month	Number of Sunspots	Month	Number of Sunspots
July 1872	106	July 1897	28	Jan. 1915	23
Jan. 1873	87	Nov. 1898	31	Feb. 1915	42
Aug. 1873	68	Feb. 1899	9	Dec. 1916	53
Nov. 1873	55	Mar. 1899	18	Mar. 1917	67
Apr. 1874	32	Aug. 1901	1	Oct. 1917	51
Nov. 1875	18	Dec. 1901	0	May 1918	77
Mar. 1876	31	Feb. 1902	0	July 1919	65
Sept. 1876	10	Oct. 1902	16	June 1920	39
Nov. 1876	10	Dec. 1902	1	July 1926	52
Oct. 1877	7	Sept. 1904	30	June 1928	91
Nov. 1877	14	Aug. 1905	59	Apr. 1929	53
June 1879	5	Mar. 1906	64	July 1931	17
July 1880	22	June 1906	63	Mar. 1932	11
Sept. 1882	58	Aug. 1906	48	Nov. 1932	8
Feb. 1884	87	Nov. 1907	62	May 1933	3
June 1887	16	June 1908	48	Aug. 1933	0
Mar. 1888	8	Dec. 1909	54	Sept. 1935	42
July 1889	10	Aug. 1911	4	Jan. 1936	60
Nov. 1889	0	Nov. 1911	4	Jan. 1937	122
Nov. 1892	65	Mar. 1912	5	June 1938	98
May 1894	101	Apr. 1913	0	Sept. 1938	90
Apr. 1895	77	July 1914	5	Feb. 1939	77
Feb. 1896	57	Dec. 1914	22	Apr. 1939	109

The dryest year was 1922 (29.3 inches) closely followed, however, by 1825 (29.7 inches) and 1881 (30.2 inches). The wettest year was 1867 (61.2 inches) which included the wettest June (11.0 inches) and the wettest August (15.8 inches). The next wettest year was 1859 (58.1 inches), followed by 1865 (56.3 inches), 1841 (55.5 inches) and 1873 (55.3 inches). The three years of greatest precipitation occurred in the period near the Civil War. During the twentieth century there have been only three years with more than 50 inches of precipitation; these are 1906 (51.9 inches), 1911 (51.4 inches), and 1933 (51.4 inches). While the average yearly temperatures ranged only from 48°.8 to 58°.1, a spread of 19%, the precipitation was much more volatile, from 29.3 inches to 61.2 inches, a range of 109%, nearly six times as great a relative variation.

TABLE J

YAERS WITH YEARLY AVERAGE OVER 100 SUNSPOTS DAILY

Year	Daily Average	Rank	Year	Daily Average	Rank
1769	106		1836	122	8
1770	101		1837	138	3
			1838	103	
1778	154	1			
1779	126	6	1848	124	7
1787	132	4			
1788	131	5	1870	139	2
1789	118	9	1871	111	
			1872	102	
			1917	104	
			1937	114	10
			1938	110	

In variability the monthly sunspot numbers markedly exceed both mean precipitation and monthly temperature averages. The figures are rather interesting and will bear some review both on the basis of (a) yearly averages and (b) monthly maxima. There have been seventeen years during 191 in which the average number of sunspots per day exceeded 100. From Table J it may be observed that these occur in eight groups of years.

The greatest average number of sunspots per day in Table J is 154 in the year 1778 (during the Revolutionary War) and the second largest number is 139 in 1870, when Germany and France had a brief war. The year 1917, when America entered World War I, is on the list, but not among the first ten years in average number of sunspots. The year 1848, which was a time of widespread revolution in Europe due to the influence of Karl Marx and the development of machinery, ranks seventh with 124 sunspots. The stormy political quarrels over the American Constitution reached a climax in the period 1787-1789 and these were also stormy years on the sun. In each of the respective groups of years the number of sunspots decreased as the years advanced except in 1836-1838 where the central year had the greatest number. Seven of the years in Table J were in the second half of the 18th century and only seven during all of the 19th century. Since 1870, which was about the time when earth's temperatures began a long-range climb and precipitation began to fall off, the sunspots have been generally of lower numbers than formerly, using Table J as a criterion.

When a tabulation (Table K) is made of the months in which the various cycles of sunspots reach their maxima, there are 18 such months during the 191 years—1749-1939. These figures are even more informative than those of Table J.

TABLE K

MONTHS WITH SUNSPOT MAXIMA FOR A CYCLE

Month	Number of Sunspots	Rank	Years Interval (Mean)		Month	Number of Sunspots	Rank	Years Interval (Mean)	
Nov. 1749	159	6	12		Dec. 1848	160	5	12	
May 1761	107		8		July 1860	117		10	24
Oct. 1769	158	7	9		May 1870	176	3	14	
May 1778	239	1	9		Jan. 1884	92		9	21
Dec. 1787	174	4	17		Aug. 1893	129	10	12	
Oct. 1804	62		13		Nov. 1905	107		12	23
Mar. 1817	96		13		Aug. 1917	154	8	11	
Apr. 1830	107		6		July 1928	98		9	
Dec. 1836	206	2	12		July 1937	145	9		

The most prolific efflorescence of sunspots (239) occurred in May, 1778; this year had the highest average of any year. The second greatest number was 206 in December, 1836; and the third, 176, in May, 1870. The most remarkable characteristic of the sunspot numbers in Table K is their sequence as we advance from one maximum to the next. The year 1749 is the first under review and we do not have information as to the consecutive monthly numbers prior thereto. Starting therefore

with the next maximum, 107, in May, 1761, the numbers at the respective maxima increase to a climax or crescendo of 239 in May, 1778, then decrease to 62 in October, 1804, the lowest of all the maxima, then rise slowly to 206 in December, 1836, and decline to 117 at July, 1860. After this point the long undulations cease and the staccato beat is up, down, up, down, alternating at each maximum. Thus at about the point where Dr. Mills found a change in the trend as to earth's temperatures, we find a marked change in the undulations of the sunspot maxima. After 1860 the period from a low maximum to the next low maximum is a double sunspot period of about 23 years. This is interesting also because "in one cycle the magnetism of the sunspots is positive and in the next negative, so that it attracts the opposite end of the compass needle. Thus 23 years elapse before sunspots recur to the same kind of magnetism and at the same point in the cycle." (Stetson). In passing it may be noted that of the 18 maxima, twelve occurred during the second half of the calendar year. The numbers are small, however, and this may be but a coincidence.

Other observers have had some success in relating earth's temperatures and precipitation to the sunspot cycle. Thus Dr. H. T. Stetson, in reviewing the literature, points out that "Mr. H. H. Clayton has found in the snowfall records at the Blue Hill Observatory in Massachusetts 40% more snow at sunspot maxima than at sunspot minima." Dr. Stetson continues, "While the North Atlantic shows 10 to 20% more precipitation in years of greater sunspots, the eastern half of the United States is in the region where the rainfall is actually less during maximum activity of the sun." Thus the fact that Philadelphia, Pa., is on the boundary between the North Atlantic and the eastern United States may explain why the results herein as to precipitation have been so inconclusive. To quote further, "Mr. Clayton has been rather successful in predicting the mean temperatures in New Haven, Connecticut, from tabulations of a 68 year period in sunspots." Thus it is evident that at least one investigator is so firmly persuaded of the relationship of sunspots and earth's temperatures that he is using the periodicity of the one as a basis for prophesying the future course of the other.

In all the 204 months of excessive wetness and excessive dryness which appear in Tables G and H, there were only two in which the number of sunspots was at a peak. These are July, 1860, (dry) and May, 1870, (wet). It is of interest, however, to compare the numbers for the respective cycles of Table K. Of these there are eleven in the years 1820 and later for which the precipitation at Philadelphia, Pa., is available. The comparison appears in Table L.

There may be significance in the fact that prior to July, 1860, the sunspots were more numerous at times of extreme relative wetness (4 periods), while thereafter in five of the seven periods the reverse was

the case. We have previously noted that somewhere near the time of the American Civil War (a) a long-range period of rising temperatures began (which other investigators have found to be world-wide) and (b) the nature of the sunspot cycles changed to a sharper rhythm, losing the longer undulation which formerly covered several cycles.

TABLE L

COMPARISON OF SUNSPOT NUMBERS WITH PRECIPITATION AT PHILADELPHIA, PA.

Period	—Extreme Wetness— Number of Months (Table H)	Average Number of Sunspots	—Extreme Dryness— Number of Months (Table G)	Average Number of Sunspots
Jan. 1820-Mar. 1830	8	26	9	20
Apr. 1830-Nov. 1836	7	50	5	32
Dec. 1836-Nov. 1848	13	64	1	45
Dec. 1848-June 1860	17	54	6	51
July 1860-Apr. 1870	20	38	2	82
May 1870-Dec. 1883	16	53	11	38
Jan. 1884-July 1893	6	31	9	41
Aug. 1893-Oct. 1905	14	31	10	40
Nov. 1905-July 1917	16	35	5	51
Aug. 1917-June 1928	6	62	7	39
July 1928-June 1937	9	35	3	44

It may be asked what the results would be if a similar study were made for temperature from the data of Tables C and D. Unfortunately there were only 27 months listed in Table C and when broken up into eleven periods (the number for which there are corresponding data in Table D) the numbers are too small to have significance. As a matter of fact, however, it does happen that for each of the five periods prior to July, 1860, the average number of sunspots was greater for the months of excessive relative cold, while in the six subsequent periods there were three more numerous in the "hot" months and three in the "cold" months. This, therefore, does tend to parallel the above findings as to precipitation and sunspot numbers.

(To be continued)

The Moon and the Cause of Optical Illusions

By ARTHUR H. KORN

The question discussed in this paper is one with which most persons are familiar and for which various explanations have been offered. It has been considered by astronomers, physicists, physiologists, and psychologists. This paper stresses the psychological aspect and hence may justifiably be added to the extensive literature without merely reiterating theories presented earlier. EDITOR.

Who hasn't seen a moon of overnatural size peeking at him from behind roof-tops, church steeples, mountain peaks, and tree tops? Man has grown so accustomed to this phenomenon that he can hardly believe

that accurate micrometric measurements prove that the Moon, when near the horizon, is actually not larger than in any other position in the sky.

This was a much discussed topic during the latter part of the last century. The opinion was advanced that the Moon appears to be larger when near the horizon because in that position it can be compared in size with other objects, such as trees and buildings. But that is no explanation, for we may ask next why do such objects of comparison make the moon appear to be larger and not smaller, since this latter would seem far more plausible? The phenomenon also has been ascribed to atmospheric refraction of light. The truth is that the refraction produces the opposite effect. A vertical shortening of the lunar disk, making it appear oval is the result. No less a person than the great *Gauss* voiced the opinion that the seemingly different sizes of celestial bodies at different heights above the horizon were due to the more or less steep angle of the observer's line of vision. We can only retort with a shrug of the shoulder: "How come?"

It is obvious that the phenomenon is due to an optical illusion, more exactly to an erroneous estimate of sizes.

Let us reflect a minute as to how we arrive at an estimate of sizes. The absolute size of the image of an object produced on the retina of our eye does not convey to us its true size. Thus, the image produced on our retina by a dog standing 10 feet away from us will be much larger than the image of a man 200 feet away. In spite of it the thought that the dog was taller than the man would never enter our mind. Distance, perspective, is a factor in the process of judging sizes. We estimate the size of the man by comparing him with other objects at the same distance. Likewise, we perceive depth of space, or perspective, when objects, known to us by experience to be of equal size, are registered on our retina as being of different sizes. The ability to judge sizes is acquired by a painstaking, long chain of experience only. Our judgment of sizes and what is basically the same thing, our sense of perspective, will be the more accurate the more objects of comparison we have, at various distances. Whenever such objects are practically absent, for instance, on the high seas, it will be very difficult to judge sizes and distances. On the high seas or on a barren plain, distances are nearly always greatly underestimated. We may say that at first glance a scene appears to be flat, and only when we scan the image, and happen to descry houses and trees in the distance, do we perceive its depth.

Let us consider two spheres of unknown actual sizes, located at different points of a scene, which are registered on our retina as being of equal size, while the scene permits a good perspective. We perceive one of the two spheres as being farther in the distance. Immediately we attribute to it a larger diameter. The process is, therefore, this: De-

pending on whether or not a landscape permits depth vision to a fuller or limited degree, we attribute to an object of unknown actual dimensions, located in the background, a greater or lesser size.

With this we have already found the solution of our lunar phenomenon. When we see the Moon in surroundings which give a good perspective, near the horizon, we attribute to it a larger diameter. But when we see it riding high in the sky, where there are no objects to awaken in our minds a sense of spatial depth, it will appear disproportionately smaller.

With the knowledge gained, we are even able to make definite statements as to the circumstances under which the Moon will appear particularly large. In general, the higher the Moon is in the sky the smaller it will appear to be. Depending on whether we see the Moon in scenery which permits a good perspective, or in one less conducive to depth perception, it will impress us more or less profoundly. A careful observer here may raise the objection that he has found the Moon, even at the same height and in the same surroundings, some times more at other times less impressive. In fact, I, too, have found the Moon, under seemingly the same conditions, some times of really frightening magnitude. Today I know why. In the dark of night the scenery is only outlined as a flat picture against the sky, a mere *silhouette*. But when the Moon happens to rise in the evening twilight, at a time when the individual objects of the landscape have not yet lost their bodily three-dimensional character, and we still are under the influence of depth of space—then indeed the Moon seems to say especially forcibly: "You! —It's you I mean."

At the same time, we have clarified another peculiar phenomenon, a question which Man has always felt to be closely related to the aspect of the Moon discussed. We do not perceive of the vault of the heavens as that of a hemisphere, but as that of a flattened dome, a spherical segment. This apparent flattening has even been more closely determined at different hours of the day and at different places on earth. The explanation now appears to be so simple, almost self-explanatory. Around the horizon, terrestrial objects convey to us a more or less accurate perception of the infinite depth of space. On the other hand, the nothingness, the emptiness of space above us, cannot produce such an impression in our minds. Perception of depth, in fact, is nothing but judgment of distance, through a comparison of familiar objects at different distances from the observer.

Let us sum up briefly! When we say that the Moon near the horizon appears to be larger than when it rides high in the sky, we are judging its size. The judging of sizes is synonymous with gauging distances. Neither process is possible without the other. The gauging of distances is based on the fact that objects, which by experience we know to be of equal size, produce images of different sizes on our retina. In such

a case we say that the object which we see as the smaller one is farther away from us. If we underestimate the size of an object of unknown actual dimensions, the reason is that we have underestimated its distance. On the other hand, if we overestimate its size, we have overestimated its distance.

Now in the last analysis, what is it that makes us perceive the depth of space? Evidently, this sense is not limited to certain definite objects; the most diverse objects can produce this feeling. Ultimately, an abstract something must be conveying to us the sense of distances.

Of all objects, it is perhaps the average man with whose size we are most familiar.

Let us assume that we stand on a wide, barren plain. On this plain, we see two men, standing at different points. They are the only objects to provide a perspective. Then we estimate their respective distances from us by comparing the two men with each other.

What do we mean by "comparing"? *To compare* means to place the two men next to each other to ascertain which one is taller, and how much taller one is than the other. In fact, this is what we actually do in our minds. Mentally we draw a line from the sole of the foot of one man to that of the other one, and a second line from the top of the head of one man to the top of the head of the other one. If these two lines are parallel, we say that both men are at an equal distance from us. But if the two lines appear considerably non-parallel, we conclude that the man who appears to be the shorter one is the one standing farther from us.

Now we have found that abstract something that makes us perceive the depth of space. It is the non-parallelism of two or more lines. Let us not be afraid to use this finding to formulate a general rule: Every non-parallelism produces in our minds a sense of spatial depth. This sense enables us correctly to judge spatial dimensions. Yet, it also misleads us when the scene happens to be not a room but the surface of a sheet of paper.

Thus we have now encountered a third problem: *optical illusions in drawings*. Whenever we see non-parallel lines on a sheet of paper, our minds associate them with spatial depth. We perceive abstract shapes drawn on the paper as located at different distances, and according to this simulated spatial depth we attribute to them sizes other than they actually possess.

This can briefly be elucidated by an example: Think of a drawing showing two circles of equal size; both have radial lines, but on one these lines point inward, on the other they point outward. The non-parallel lines induce the spectator to see a perspective,—in this case to see a tunnel. The spot where the lines diverge most is its foreground, and the spot where they most converge is its background. One circle, therefore, represents the near entrance of the tunnel tube, the other

one the far opening. In accordance with the thus perceived greater distance, we see the circle with outward-pointing radial lines as being of a larger diameter.

Optical illusions, as long as they are not based on color effects, can thus conveniently be explained. Of all the attempts at giving an explanation, perhaps the theory advanced by *Wundt* in the past century was the one to receive most attention in scientific circles. He observes a certain given picture and then tries to explain the optical illusions that is, the wrong judgment of sizes, with the movements taking place on the retina. He forgets, however, that Man, just like a newborn infant, is absolutely unable to interpret an isolated image in itself. Judgment of size, namely, is the interpretation of an image. We judge a momentarily perceived image in the light of our experience of the thousands and millions of images which our retina has perceived since our earliest youth. This is a psychological process, based on experience.

Although it has been admitted that perspective plays a part in optical illusions, due to unexplained reasons it has been belittled as a "secondary" factor. That it is really the Moon's seemingly greater distance that makes it appear larger near the horizon than when it is high in the sky, is, by the way, a fact which, according to *Helmholtz*, was already known to Ptolemy and to the old Arabian astronomers. I wonder why new explanations have been sought over and over again. Could it be that overzealous scientists were motivated by the desire to discount the influence of spatial depth?

Easter Reckoning Made Easy

By GEORGE W. WALKER

Last year, Easter came on April 25, its latest possible date. This year, it came on April 9, almost eactly in the middle of its range. Usually we look up the date of Easter as soon as we get a calendar for the new year. Most of us wonder vaguely how the date is determined. If our curiosity leads us to investigate the rules that are, by now, generally agreed upon, we may quickly decide that they are much too complicated to bother about, or even to try to understand. It seems much easier to look on a calendar to find out when Easter will come.

Yet the basic idea in fixing the date of Easter is, essentially, quite simple. It was accurately described, in technical terms, in this magazine last year.* Using familiar terms and ideas, and, with a little mechanical help explained below, the date of Easter can be quickly determined for any year whatever, past or future, so long as the present Gregorian rule prevails.

Astronomically, Easter falls on the first Sunday after the first full

*"Uncommon Easter Dates" by Alexander Pogo, POPULAR ASTRONOMY, Vol. 51, page 254.

moon which occurs on or after the first day of spring. In practice, however, we do not depend on observation to determine the coming of spring or the phase of the moon. Instead, we accept certain artificial, but reasonably exact, mathematical rules, which were first promulgated by Pope Gregory XIII in 1582, and, within the next few centuries, were accepted by the whole civilized world. We can make the simplest possible approach by taking the three events in order: the arrival of spring, the first full moon thereafter, and the first following Sunday.

I

We are accustomed to note, from year to year, that spring arrives, or "the sun crosses the line," on March 20 or March 21, or some such date. It would be a nuisance to bother about that small uncertainty. So the Gregorian calendar arbitrarily announces that, for the purpose of Easter calculation, March 21 in any year is to be considered the first day of spring.

From one March 21 to the next is usually 365 days, sometimes 366 days, in neither case an exact astronomical year. However, by a proper distribution of leap years, our conventional date here chosen is held reasonably close to the vernal equinox. The rule for the incidence of leap years is too familiar to need repetition here. Because this is all taken into account, we do not need to worry about it in calculating the first step in finding Easter. We simply take the date, March 21, as given.

II

The second step is the really difficult one. The moon pays no attention to our chosen starting point. It has its own quite independent schedule. The time from one new moon to the next is neither an exact number of days nor an exact fraction of the tropical year. The number of days in a true lunar month is always between 29 and 30. If we make a proper mixture of 29-day and 30-day months, we can keep fairly close to the astronomical moon. (The familiar calendar months are of no use to us here; they gave up trying to keep step with the moon away back in the days of Julius Caesar.) For short runs, a simple alternation of 29-day and 30-day months serves quite well. If the moon is at a certain phase on any given date, say on January first, it will be at very nearly the same phase, after two cycles, fifty-nine days later, that is, on March first (or February 29 in leap years). So we can reckon moon phases right through the year. This first approximation, however, is quite crude, and will not safely carry us many months.

Twelve cycles of the moon take about 354 days. Therefore any given phase of the moon will be approximated again on a date ten or eleven days earlier in the following year. And the same phase will appear again, after the thirteenth cycle, about nineteen days later than the original date. In particular, if there is a full moon early in April, which

would have to be the first one after the coming of spring, there will be another full moon, and again the first full moon of spring, on a date ten or eleven days earlier in the next year. Or if the first full moon after the equinox comes in March, there will be another about nineteen days later in the succeeding year. So we can reckon the arrival of the first full moon of spring from year to year. Table I shows how the dates shift backward and forward over a few years, as calculated by the Gregorian rule.

TABLE I

GN	Year	Date of Moon
1	1938	April 14
2	1939	April 3
3	1940	March 23
4	1941	April 11
5	1942	March 31
6	1943	April 18 (19)
7	1944	April 8
8	1945	March 28
9	1946	April 16
10	1947	April 5
11	1948	March 25
12	1949	April 13
13	1950	April 2
14	1951	March 22
15	1952	April 10
16	1953	March 30
17	1954	April 17 (18)
18	1955	April 7
19	1956	March 27

We may notice in Table I that if we alter just two dates, as indicated in parentheses, the interval of dates from one line to the next is always either 11 days earlier or 19 days later. This does not necessarily mean annual intervals of 354 days or 384 days, respectively, because in some cases we have extra days with leap years. The rule here is really easier to apply than it would be if we had to reckon exactly 354 or 384 days. Indeed, the rule is very simple. If by its use we can keep in step with the moon through centuries as within nineteen years, we can work out a very simple procedure for reckoning Easter. Any date found for first full moon of spring in one year would inevitably be followed in the next year by a date either eleven days earlier or else nineteen days later, whichever falls within the first full month after vernal equinox.

As it happens, we can do even better than that. If we carry out Table I just one step further, we come back very near to the starting place; and it happens that if we will slide across the line to the exact starting place again for 1957, we will find ourselves almost exactly in the original phase of the moon. In 19 years, the moon completes almost exactly 235 cycles. So, to extend or table, all we need to do, really, is to add another column of years, reading downward from 1957 to 1975, and then another from 1976 to 1994, and so on; and we have a repeating table of dates that will serve us well for centuries ahead.

There is a simple way of describing all the years that will be asso-

ciated together in this way with a given date for the first full moon of spring. For instance, all the initial years of the columns, those which have the full moon on April 14, are exactly divisible by 19. We may call these *first-row* years. Then, all that belong with April 3, when divided by 19, leave the remainder 1. These are *second-row* years. In general, the remainder after dividing any year number by 19, with one added, will indicate the row in which that year will be found. This figure is also called the "Golden Number" of the year in the traditional mode of Easter calculation. The Golden Number of any year can be found by dividing the year number by 19, throwing away the quotient, and adding 1 to the remainder. The Golden Number of 1944 is 7.

The cycle of 235 moons in 19 years is remarkably good, but not quite good enough for long range calculations of Easter; and the Gregorian rule provides for occasional steps which sometimes have the net effect of continuing into a second cycle of 19 years the rule of making every change either 11 days earlier or else 19 days later. The second time around, the whole set of dates is shifted, in general, by one unit. Except where the pattern runs out of the boundary, the mutual relationships remain just the same.

Such an adjustment really should be made at least once every two or three centuries to keep our calendar in step with the moon. As a matter of fact, the Gregorian rule provides for making the shift always just before a year whose number is divisible by 100, like 1900, for instance; and decides whether to shift or not in each case according to two independent rules. On the centuries which do not bring century leap years, a shift forward is designated; but, also, eight times in every 25 centuries, usually at 300 year intervals, a backward shift is provided for, which either cancels the forward shift otherwise authorized, or even carries the pattern a step to the rear. The net result in the long run is 43 steps forward in every 100 centuries.

All of this becomes extremely complicated when it is put into calculation to find the date of Easter for any particular year. Wherefore the author has accepted the idea of an unpublished cylindrical diagonal table of Golden Numbers communicated, in private correspondence, by Dr. Alexander Pogo of the Carnegie Institution of Washington, and has built himself a mechanical Moon Finder or Easter Calculator, which does mechanically, by cylinders and gears, practically all the head work for this troublesome second step in finding the Easter date.

For convenience, Table I is reversed, and the 19 different Golden Numbers are distributed (for any epoch) on the appropriate dates. The machine even takes into account the artificial crowding of dates near the end of the allowed period running from March 21 through April 18. This crowding caused the slight disturbance which we noticed when we tried to apply our 11-day or 19-day rule to the later dates of Table I. It is just one of the artificial complications in the Gregorian Easter rule.

But we can let the mechanical Easter Calculator do all our worrying about that.

Whenever we come to one of the shifts described above, at the beginning of a new century, the machine slides the whole pattern along a notch, keeping the configuration, in general, intact. The proper times for shifting the pattern are plainly indicated by a concurrent list of century numbers. We find a shift indicated, for instance, between the 18 hundreds and the 19 hundreds; and we find the next one after the 21 hundreds, just as we are about to turn to the year 2200.

There must be thirty shifts of the pattern of Golden Numbers before the original arrangement is seen again. This takes about 70 centuries. On the other hand, there are 43 steps in making the rounds of the 100 century numbers, before the total pattern of century numbers exactly repeats itself. The Easter Calculator provides the cylinder bearing the

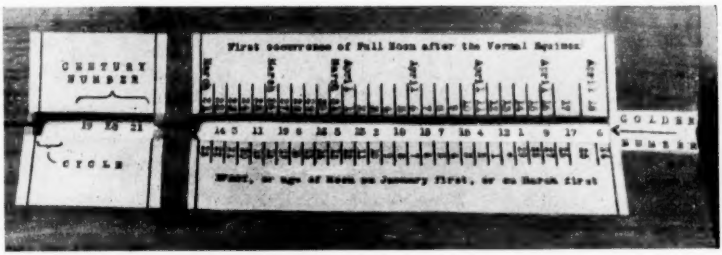


FIGURE 1

Top view of the "Moon Finder" or "Easter Calculator" in working position, with the cylinder set for the present series of three centuries. The data given in Table I can be read directly from the machine in this position. Compare the corresponding row in Table IV.

Golden Numbers with 30 cogs, which mesh with the 43 cogs of the surrounding and riding cylinder bearing the century numbers. Thus each cylinder repeats itself after the proper number of shifts.

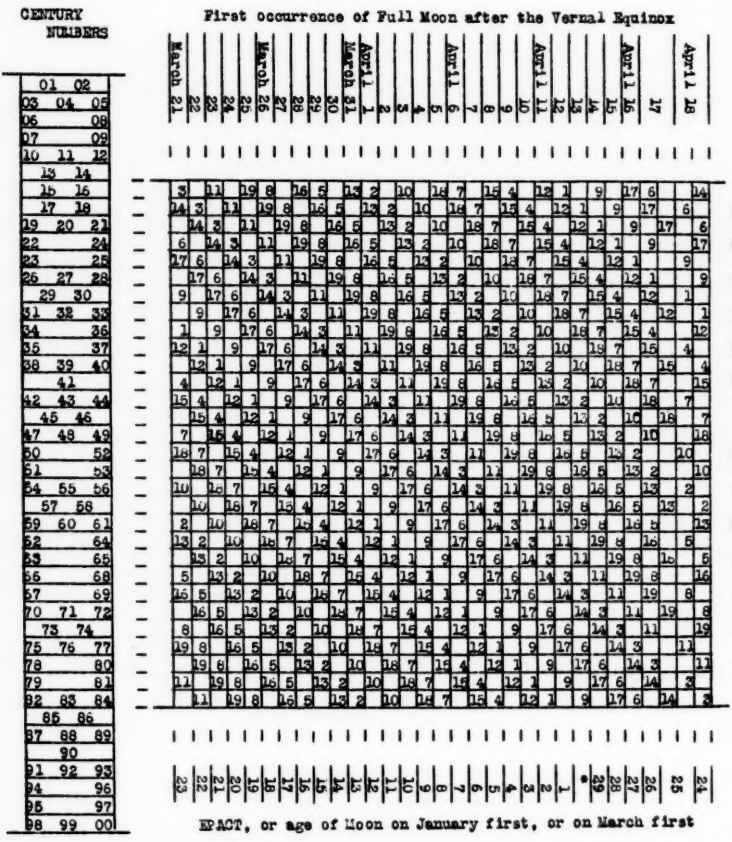
It is plain that the numbers, 30 and 43, are not commensurable. Consequently, every time either cylinder comes around again to its starting place, it is met with a different set of figures on the other cylinder. It takes 43 revolutions of the smaller cylinder, or 30 of the other, to restore everything to the starting position. This means the passing of 3000 centuries, or 300,000 years.

In the course of this long-range circuit, the number of the particular cycle that is currently being traversed by the cylinder of century numbers is shown, near the beginning of the cycle, by a figure which can be seen through a window in the cylinder. The figure for the present cycle is 0. Between 10,000 and 19,999, the cycle figure is 1. In general, the cycle number for any given year is the remainder found if the last four digits of the year number are cut off and the rest is divided by 30. The cycle number for the year 751944 is 15.

For convenience, the cylinder of century numbers is made detachable, so that it can be set, at once, at any cycle (by lining up the cycle number through the window), so that it is not necessary to begin from the beginning and reel off the years up to some late period that we may be interested in.

By means of this gadget it is quite easy to find the date of the first full moon of spring for any year that may be assigned. First, note the cycle and the century number of the year, and set the cylinders accordingly. Then figure the golden number of the year. Look for that golden number on the machine, and it will give directly the date desired.

TABLE II



For separate study, the figures that are found on the cylinders are presented in display form in Table II, in which the two parts are lined

up with each other as for cycle 0, for the present epoch. It is easy to read across from century number 19, to see, for instance, that the golden number 7 (for 1944) indicates that the full moon that we are interested in this year falls on April 8. For any year between 1500 and 8499 A.D. Table II is almost as convenient to use as the machine which has the same data on cylinders.

The epact numbers which appear on the Mechanical Moon Finder or Easter Calculator, and in Table II, are given mainly for historical reasons; but also to show how the wrinkles at April 17 and 18 (due to compressing the period into 29 days) are constituted. A study of the columns at the right edge of Table II will show by what device the three epacts, 24, 25, and 26, are crowded into two full moon dates, April 17 and 18.

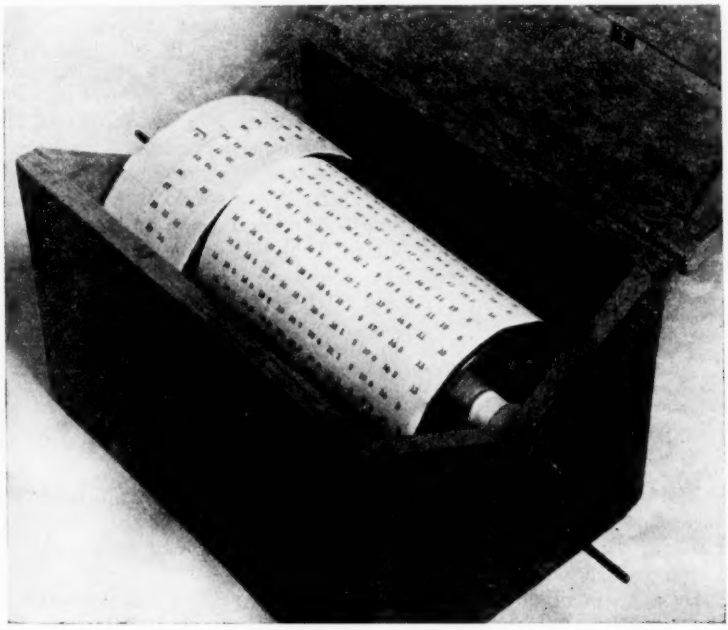


FIGURE 2

The working cylinders of the "Easter Calculator" shown with the cover lifted. Notice the Cycle Number "0" showing through the little window to the left of the Century Numbers "01" and "02." This Cycle Number shows that the cylinders are set for the present period of 100 centuries.

"Epact" may be defined roughly as the age of the moon on January first, or, for that matter, on March first, which is taken to be the same. If the moon is 5 days old on March first, it will be 5 days old on March

30, and it will take it until April 8 to be 14 days old, that is, to bring it to full moon. This is the situation in 1944, which would be listed by classical Easter calculators as a year with epact 5. Our mechanical epact calculator so lists it too.

III

We come, lastly, to another fairly simple part of the process of calculating Easter. In the second step, we fixed the date of the first full moon of spring (or, rather, the artificial full moon of an artificial spring). All that remains now is to figure out how soon thereafter comes a Sunday.

It is obvious that Sundays and dates are related. If March 22 is Sunday, so must also be March 29, and April 5, 12, and 19. Similarly, if March 23 is Sunday, so must also be March 30, and April 6, 13, and 20.

Neglecting the disturbance of February 29 in leap years, which simply means that we have to deal with the first two months of any leap year separately, we may say that there are just seven kinds of years that we need to be concerned with here. One kind has Sundays on March 22 and the other dates listed with it above; another has Sundays on March 23 and its associated dates; and so on. Classically, these seven kinds of years are labelled with "Dominical Letters" from A to G. The Sunday dates that we are interested in for these several kinds of years are all given in Table III. The first line of Table III applies to our present year.

TABLE III

DL	SUNDAYS:
A	March 26, April 2, 9, 16, 23
B	March 27, April 3, 10, 17, 24
C	March 28, April 4, 11, 18, 25
D	March 22, 29, April 5, 12, 19
E	March 23, 30, April 6, 13, 20
F	March 24, 31, April 7, 14, 21
G	March 25, April 1, 8, 15, 22

The only task that remains for us now is to determine which of the seven kinds of year describes any assigned year that we may be interested in. In other words, we want a rule for determining the Dominical Letter for any year.

By a fortunate circumstance, the leap year cycle of 400 years has a total number of days which is exactly divisible by 7, so that the succession of days of the week, as well as the succession of leap years, is exactly repeated every 400 years. A table of 400 years could therefore be made, each with its proper dominical letter, and it would serve indefinitely. However, the same information can be easily given by a much more condensed table, for instance, in Table IV.

To use Table IV, we may first note the "Year Ending," namely, the last two digits of the year number. Then we may figure (by inspection) the "Quartic Number," which is the remainder when the century num-

ber is divided by 4. Then we simply look in the row indicated by the quartic number under the column containing the year ending, and we find the dominical letter for the year. For example, 1944 has the year ending, 44. The quartic number (remainder after dividing 19 by 4) is 3. The table shows that the dominical letter under 44, opposite 3, is A. The dominical letter for any year can be found as simply as that. Then Table III can be used to find the first Sunday following the date discovered in the second step for the first full moon of spring; which is the Easter date desired.

Table IV, as given here, is based on a table appearing in the Book of Common Prayer of the Protestant Episcopal Church of the U.S.A. Doubtless the same table, or similar tables, are found in many other sources as well. It should be added that Table IV, as given here, applies only to the last ten months of any leap year (which is all we are interested in anyway, for calculating Easter). If for any other purpose, the Dominical Letter is desired for the earlier part of a leap year (January and February), Table IV can still be used. The year ending number just has to be shoved ahead into the blank space which precedes it here in Table IV.

TABLE IV

	Year Ending						
	00	01	02	03		04
05	06	07		08	09	10
11		12	13	14	15	
16	17	18	19		20	21
22	23		24	25	26	27
	28	29	30	31		32
33	34	35		36	37	38
39		40	41	42	43	
44	45	46	47		48	49
50	51		52	53	54	55
	56	57	58	59		60
61	62	63		64	65	66
67		68	69	70	71	
72	73	74	75		76	77
78	79		80	81	82	83
	84	85	86	87		88
89	90	91		92	93	94
95		96	97	98	99	

Quartic Number	Table of Dominical Letters						
0 (e.g. 1600, 2000)	B	A	G	F	E	D	C
1 (e.g. 1700, 2100)	D	C	B	A	G	F	E
2 (e.g. 1800, 2200)	F	E	D	C	B	A	G
3 (e.g. 1900, 2300)	A	G	F	E	D	C	B

Now, just to see if we have caught up all the loose ends, let us follow the procedure here given to find on what day Easter will come a million years from now (on the very improbable assumption that the Gregorian rule will still be in force by then). The year 1001944 is in cycle 10 (remainder after dividing 100 by 30). The century number is 19. The golden number is 18 (1 more than the remainder after dividing

1001944 by 19). The Easter Calculator is set for cycle 10 and century number 19, and it shows that golden number 18 brings the first full moon of spring to the date April 17. Next we find from Table IV (for quartic number 3 and year ending 44) that the dominical letter for 1001944 is A. Consulting Table III, we find that the first Sunday after April 17 in the row for dominical letter A is on April 23. Therefore Easter in 1001944, according to the Gregorian rule, will come on April 23. The whole complicated process has been smoothed out, with the help of the mechanical Easter Calculator, so that it is fun to calculate Easter dates.

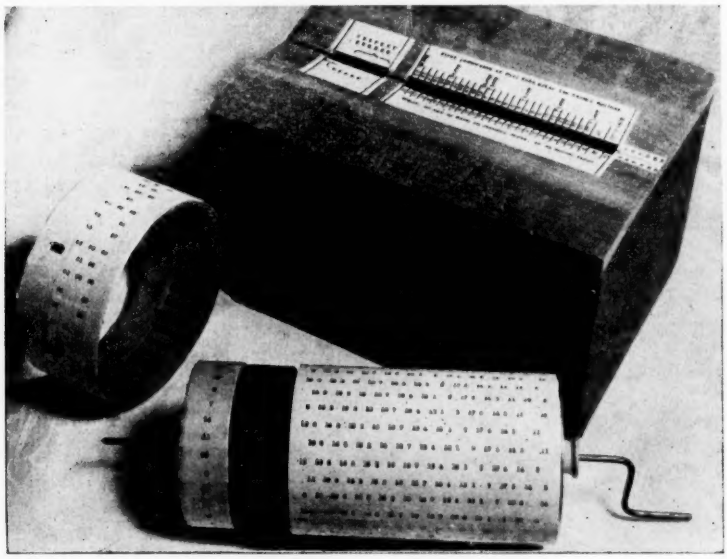


FIGURE 3

The three parts of the "Easter Calculator" shown separately. Century Numbers are on the hollow cylinder at the left. The solid cylinder has Golden Numbers on its main surface, Cycle Numbers in the margin at the left.

We should, perhaps, say again, for emphasis, that what we are doing is finding the next Sunday after an artificial full moon which comes soonest after an artificial vernal equinox. This often gives us the same date as we would find by observation of the phases of the astronomical moon in the astronomical season of spring. The Gregorian rule will miss the exact date very seldom for many centuries or millenia to come. In a long enough time, there will be a drifting into more misses than hits. So we must take our example of a million years from now with its quota of salt.

Anybody can make a mechanical Moon Finder or Easter Calculator

for himself by rolling up Table II into two cylinders. For putting it together, if the maker does not want to go to the expense of tooled gears and cabinet workmanship, he can use old tin cans and corrugated cardboard, as the author of this article did, and the gadget should serve his needs very well.

NOTE: The figure 1365 in the footnote on page 141 of the preceding issue should be 1363.—G.W.W.

Co-ordinate Numbers for Stars et Cetera

By FREDERICK C. LEONARD

In *Contributions of the Society for Research on Meteorites*, **2**, 270; POPULAR ASTRONOMY, **49**, 214-15, 1941, a numerical designation for meteoritic falls, since called their "coördinate numbers," was described. This has proved to be so useful in connection with fallen meteorites that it is proposed to present a similar numeration for stars (and other bodies outside the solar system). The *coördinate number* of a star will be defined as a 7-digit number, of which the first 4 digits give the star's right ascension to the closest minute of time and the last 3, its declination to the closest tenth of a degree, presumably at the beginning of the nearest century or mid-century (whichever is the nearer) to the date in question (*e.g.*, for the year 1950); thus, the 1950 coördinate number of α *Lyrae* (Vega), of which 1950 $\alpha = 18^h\ 35^m.2$, $\delta = +38°\ 44'$, is 1835,387.[1] If the declination is negative (south), the final 3 digits of the number should be printed in *italics* (or *underlined* in manuscript).

It is seen that this system is essentially the same as that which has been in use for many years at the Harvard College Observatory for designating variable stars. The present system differs from the older one, however, in two respects, namely, in that it gives (1) the declination to the closest *tenth of a degree*, instead of only the closest *degree*, and (2) the coördinates at the beginning of the nearest century or mid-century—or for *any* selected epoch;[2] furthermore, it is intended for use, not just with variable stars, but with all kinds of objects outside the solar system. The coördinate number of a star (or nebula), being secularly affected mainly by the precession of the equinoxes, is evidently, then, like the coördinates themselves, of which it is composed, a variable quantity; hence, it should be understood, or specified, as pertaining to a definite date.

If, as a result of their proximity in the sky, two or more stars have identical coördinate numbers, they may still be distinguished from one another, simply by means of serial numbers appended to their common coördinate number and assigned in the order of right ascension, declination, date of discovery, or what not, thus: 0533,*054*:1, 0533,*054*:2, 0533,*054*:3, *etc.*[3]

For what purpose is the coördinate number of a star? The answer

is that it serves as a means not only of numeration or identification but also of location. Since the number shows at a glance the (approximate) right ascension and declination of a star, it may be employed as a substitute for these quantities in writing, in printing, in reference, and at the telescope, except when the requirements of the case demand that the right ascension and the declination be known to, or be quoted with, a higher order of accuracy than the closest minute of time and the closest tenth of a degree. Rarely, if ever, do the coördinates of a star have to be known more precisely, for setting even the largest equatorial telescope; the important thing is that they should be *up to date* and so *currently correct* to the closest minute of time and tenth of a degree, as they will be, in almost every instance, if the coördinate number of the object has been evaluated for the beginning of the century or mid-century nearest to the date in question.

The coördinate number, in lieu of the right ascension and the declination of a star, is, therefore, an indispensable item for use at the telescope; it is, moreover, a time-saver and a space-saver in writing and printing lists, tables, and catalogs of stellar bodies, in labeling photographic plates, and in making all sorts of records and references that do not call for a high degree of accuracy in the citation of positional data.

NOTES

[1] For facility in reading, a comma is inserted between the first 4, and the last 3, digits of the number.

[2] The Harvard numbers for variable stars all are made up of coördinates for the year 1900.

[3] The first two numbers are the 1950 coördinate numbers of θ^1 and θ^2 *Orionis*.

DEPARTMENT OF ASTRONOMY, UNIVERSITY OF CALIFORNIA, LOS ANGELES

Photographing a Penumbral Eclipse

By ALICE H. FARNSWORTH

The evening of 1944 February 8-9 was clear in South Hadley and we were thus encouraged to test the validity of a statement[1] we had read: "The penumbral lunar eclipse of 1944 February 9 . . . will be beyond the reach of the unaided eye, but it could be observed photographically."

The Moon was to enter the penumbra at $3^h\ 18^m$, and be closest to the physical umbra (at a distance of 15′.1) at $5^h\ 15^m$ U.T., the magnitude of the eclipse being —0.51 in units of the Moon's diameter.

A box attached to the 8-inch refractor serves as carrier for a 3¼ by 4¼-inch commercial plate-holder. Eastman 50 plates, sensitive to yellow as well as blue, and developed in D 11 at 65° F., were used.

Miss Helen Pillans assisted in both the observing and testing programs.

[1] Pogo: "The Four Penumbral Eclipses of 1944," POPULAR ASTRONOMY, **51**, 354, 1943.

Experiment during the early evening showed (1) that 1ˢ with aperture reduced to 4 inches and no color-filter gave a very dense image of the Moon surrounded by much field fog, (2) that ¼ˢ with 2-inch aperture gave a clear field around a still heavily overexposed image, (3) that ½ˢ with 2-inch aperture and a Wratten minus-blue filter before the plate was too short, and, finally, (4) that an exposure of 3ˢ with this latter arrangement was satisfactory.

The details of three pairs of exposures are indicated here:

Plate Nos.	Exp. Time s	U.T. h m	H. Angle h m	Remarks
164	3	3 10	—1 48	Moon normal
165	4	3 16	—1 42	Moon normal
166	3	4 18	—0 42	Moon half in
167	4	4 19	—0 41	Moon half in
168	3	5 16	+0 14	Moon closest to umbra
169	4	5 18	+0 16	Moon closest to umbra

Plates 164, 166, and 168 were developed together for 5 minutes; 165, 167, and 169 together for 4¼ minutes.

Visually, even to a prejudiced eye or opera-glass, there was no suspicion of shading on the penumbra-immersed southern limb of the Moon at mid-eclipse. The first clause of the statement in paragraph one is amply confirmed under favorable conditions.

Confirmation of the second clause is indubitably present in the photographs but not to the merely casual eye or to a degree that is thought adaptable to reproduction in this report.

FIGURE 1

A sketch of the Moon's image, actual size, showing the test areas: U, northeast of Mare Crisium, and L, southwest of Tycho.

The contrast between the normal Moon (164 and 165) and the eclipsed Moon (168 and 169) was made clear when the following test was applied. The images of the Moon, one inch in diameter, on Plates 165 and 169 were covered by diaphragms which exposed to view two small areas only. These were named "U," for Upper, and "L," for Lower (see Fig. 1). These areas were so chosen that U had been constantly under full illumination from the Sun, whereas L had suffered

eclipse by the penumbra. The two diaphragmed plates were laid on a viewing-frame and ten students, using a hand-magnifier which brought both U and L into view at once, gave estimates for each plate as to the comparative blackening of U and L. A similar test of Plates 164 and 168 was made with eight students. Results were as follows:

Plate	Moon	Total No. of estimates	L denser than U	U denser than L	L = U
165	Normal	10	10	0	0
169	Eclipsed	10	0	7	3
164	Normal	8	8	0	0
168	Eclipsed	8	0	4	4

These figures show (1) that all observers agreed that area L is a normally brighter (denser on the negative) region than U, (2) that all observers agreed that the brightness of U relative to L was less at mid-eclipse, and (3) that 11 out of 18 students estimated that L then appeared actually less bright than U, the remaining 7 that L had merely been reduced to equality with U. Two independent series of estimates (made by the writer and Miss Pillans) of all exposures (including the four experimental plates with enormous range of density) on ten plates thoroughly mixed and with all labels covered, showed L brighter than U on 8 plates, and reduced to equality or slightly reversed on the 2 mid-eclipse plates.

In the absence of densitometer measures, these estimates provide qualitative evidence of the photogenic quality of a penumbral eclipse of magnitude —0.51. Another and better chance to photograph a penumbral eclipse occurs on July 5-6, 1944, when the negative magnitude is only —0.43 and the closest approach of the Moon to the umbra is reduced to 13′.8. The brighter, northern limb will again be immersed and mid-eclipse occurs at $4^h\ 39^m$ U.T., July 6.

John Payson Williston Observatory, South Hadley, Massachusetts, March, 1944.

The Distribution of the Stars as a Function of Spectral Class

By RALPH B. BALDWIN

Occasionally an astronomer has need of knowledge regarding the actual space densities of stars as a function of spectral type. Information of this type has been published by Shapley,[1] by Becker,[2] and by Shapley and Cannon.[3]

It is interesting to see how the work by Shapley and Cannon, based on the Henry Draper Catalog, compares with results derived from the latest determinations of the luminosity function.

Luyten[4] has derived a luminosity function through an intercomparison of his own work and that of van Maanen,[5] Seares,[6] and van Rhijn.[7] This luminosity function has been adopted here. It is a relationship

between star density in terms of the number of stars in the southern hemisphere nearer than five parsecs, and absolute photographic magnitude on the international scale.

The relationship between visual absolute magnitudes and spectral classes was taken from "The Masses of the Stars"[8] and the relationship between temperature and spectral class is according to Kuiper.[9] The formula:

$$I = M_p - M_v = 7200/T - 0.64$$

was used to convert Russell's visual magnitudes to photographic magnitudes.

Armed with these data Luyten's final luminosity function was plotted and the relative numbers of stars determined for each absolute photographic magnitude range corresponding to each half spectral class, *e.g.*, for F0 to F5, for dG5 to dK0, etc.

The luminosity function goes only to photographic magnitude —1.0 while a B0 star is about —3.1. In making this computation it was assumed that the ordinate to the luminosity function at class B0 was about 0.0005 on Luyten's scale.

TABLE 1

THE LUMINOSITY FUNCTION (LUYTEN)

M_p	No.	M_p	No.	M_p	No.
— 1	0.001	+ 8	1.24	+17	1.95
0	0.023	+ 9	1.49	+18	1.21
+ 1	0.11	+10	1.81	+19	0.51:
+ 2	0.20	+11	2.11	+20	0.11:
+ 3	0.29	+12	2.38	+21	0.02::
+ 4	0.40	+13	2.65		
+ 5	0.57	+14	2.84		26.994
+ 6	0.76	+15	2.84		
+ 7	0.97	+16	2.51		

TABLE 2

SPECTRAL CHARACTERISTICS

Sp.	T	M_v	M_p
B0	25,000°K	— 2.8	— 3.1
B5	15,500	— 0.7	— 0.9
A0	10,700	+ 0.7	+ 0.7
A5	8,500	+ 1.7	+ 1.9
F0	7,500	+ 2.5	+ 2.8
F5	6,500	+ 3.2	+ 3.7
dG0	6,000	+ 3.9	+ 4.5
dG5	5,400	+ 4.7	+ 5.4
dK0	4,900	+ 5.5	+ 6.3
dK5	4,150	+ 6.7	+ 7.8
dM0	3,600	+ 8.4	+ 9.8
dM5	2,850	+11.6	+13.5
gG0	5,200	+ 0.7	+ 1.4
gG5	4,600	+ 0.7	+ 1.6
gK0	4,200	+ 0.5	+ 1.6
gK5	3,600	+ 0.1	+ 1.5
gM0	3,200	— 0.2	+ 1.3
gM5	2,840	— 0.4	+ 1.5

In order to derive the values of Table 3 the curve defined by Table 1 was integrated between the limits imposed by each half division in spectral class.

In addition, Shapley and Cannon[3] say that the giant G, K, and M stars occur in the ratio of 11:160:22, while the B8-A2 stars total 250 on the same scale. Since there are gaps in the Harvard scale, *e.g.*, B5 is next to B8, the above curve was integrated between the limits of B6.5 and A4, yielding 0.168 stars, of which 0.073 stars were giants. The latter broke up into 0.004, 0.061, and 0.008 for gG, gK, and gM, respectively.

In order to eliminate the giants from the luminosity function and thus establish the upper end of the main sequence it was arbitrarily assumed that the absolute magnitudes of the giants were distributed essentially on an error curve, hence 0.005 stars were eliminated from the range $M_p = 0.0$ to $+0.5$, 0.013 stars from $M_p = 0.5$ to 1.0, 0.037 stars from 1.0 to 2.0, 0.013 stars from 2.0 to 2.5, and 0.005 stars from $M_p = 2.5$ to 3.0.

With this new curve the numbers of stars in the spectral range from B5 to F5 were recomputed as shown in Table 3.

TABLE 3

THE DISTRIBUTION OF THE STARS AS A FUNCTION OF SPECTRAL CLASS

Spectral Range	No.	Spectral Range	No.
B0-B5	0.002	dM5+	11.960
B5-A0	0.040		
A0-A5	0.122	gG0-gK0	0.004
A5-F0	0.188	gK0-gM0	0.061
F0-F5	0.284	gM0+	0.008
F5-dG0	0.336		26.991
dG0-dG5	0.507		
dG5-dK0	0.659	N	0.0001
dK0-dK5	1.490	R	0.00009
dK5-dM0	2.902	S	0.000003
dM0-dM5	8.428		

The Dearborn Survey of Faint Red Stars[10] was used to determine the relative numbers of M, N, R, and S stars. It includes 8151 stars down to about 11.5 magnitude in the first published zone from —4°.5 to +13°.5 declination. Of these 2678 were listed as K5, red, or peculiar, 5322 were called M, 48 N, 36 N?, 23 R, 11 R?, 6 S, and 27 S?.

It has been assumed that all of the N stars plus half of the N? stars were actually in class N, or 66 stars; the same procedure for the R stars yielded 28 stars, while the 6 S stars plus one quarter of the S? stars total 13. The lower weight to class S? was given because of the difficulty in identifying Zr0 bands on the Dearborn plates.

Next it was assumed that the M and N stars have the same absolute visual magnitudes, the R stars are about 0.5 magnitude fainter while the S stars are 1.5 magnitudes brighter. The number of M stars was reduced to 5000 even to allow for the presence of unidentified M dwarfs.

Hence the relative frequencies of giant M, N, R, and S stars become 5000, 66, 56, and 2, respectively. From these figures are computed the values listed in the lowest section of Table 3.

The process admittedly is rough, but the correct order of magnitude should be established.

Shapley and Cannon,[3] however, have divided the spectral classes differently. This required another integration of the luminosity function, the results of which are intercompared with the Harvard frequencies in Table 4.

Column 1 lists the Harvard Spectral Classes while column 2 shows the range in spectral class on the Draper basis. Column 3 lists the ranges in spectral class when all steps are considered, which most nearly represent the Harvard ranges. Columns 4 and 5 give the numbers of stars in each class range within 5 parsecs in the southern hemisphere and in 1,000,000 cubic parsecs, respectively. Shapley and Cannon values corresponding to these are listed in column 6.

TABLE 4

THE DISTRIBUTION OF THE STARS AS A FUNCTION OF SPECTRAL CLASS

Spectral Class (Shapley-Cannon)	Spectral Range (Shapley Cannon)	Spectral Range (Baldwin)	No. of Stars Nearer Than 5 Parsecs in Southern Sky (Baldwin)	No. of Stars in Million Cubic Parsecs (Baldwin)	No. of Stars in Million Cubic Parsecs (Shapley Cannon)
B	B0-B5	B0-B6.5	0.0032	12	4.4
A	B8-A3	B6.5-A4	0.095	363	250
F	A5-F2	A4-F3.5	0.414	1578	680
dG	F5-G0	F3.5-G2.5	0.664	2536	7600
dK	G5-K2	G2.5-K3.5	1.920	7334	—
dM	K5-Mc	K3.5-M+	23.791	90882	—
gG	F5-G0	G	0.004	15	(11)
gK	G5-K2	K	0.061	233	160
gM	K5-Mc	M	0.008	31	22
			26.9602		
N	—	N	0.0001	0.4	—
R	—	R	0.00009	0.3	—
S	—	S	0.000003	0.01	—

NOTE. The data of tables 4 and 5 are given to several places although they probably are accurate to only two places.

They also state[3] that "Stars of the last two classes (dK and dM) are probably much more numerous per unit volume than dwarf stars of class G."

Wolf-Rayet stars and stars of class O as well as white dwarfs and supergiants have not been considered in this paper. Possibly the supergiants may modify somewhat the results found for class B, A, and giant G, K, and M stars because of their inclusion in the Henry Draper Catalog and hence in the intercomparison made here between giants and main sequence stars. This should be a relatively small effect.

This work has several uncertainties associated with it. It is not

known which luminosity curve is right and it is particularly difficult to ascertain the form of the extreme portions of the luminosity function. In addition the relationships between spectral class and absolute photographic and visual magnitudes are only approximate.

In spite of these drawbacks it seems that Tables 3, 4, and 5 give a rather reasonable picture of the actual space distribution of the stars according to spectral class. They do not allow for any local variations of relative frequencies of the various classes as are known to exist.

The range in frequencies is thus of the order of 10,000,000:1 between the two extremes, dM and S as subdivided by Shapley and Cannon. It is of interest to note that only about three and one-half per cent of the stars are brighter than the sun.

The data of Table 3 may be represented in another way, Table 5.

TABLE 5

THE DISTRIBUTION OF STARS AS A FUNCTION OF SPECTRAL CLASS

Spectral Range (incl.)	No. of Stars in Million Cubic Parsecs
B0-B9	163
A0-A9	1180
F0-F9	2370
dG0-dG9	4460
dK0-dK6	16800
dM0+	77800
gG0-gG9	15
gK0-gK5	233
gM0+	31
N	0.4
R	0.3
S	0.01

When the spectral classes are subdivided in this manner the range in frequencies is of the order of 8,000,000:1 between the dM stars and the S stars.

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DEARBORN OBSERVATORY,* NORTHWESTERN UNIVERSITY, MARCH 8, 1944.

*On leave to The Office of Scientific Research and Development, Applied Physics Laboratory, Johns Hopkins University, Silver Spring, Maryland.

The Planets in May, 1944

NOTE: Greenwich Civil Time is used unless otherwise stated. To obtain Eastern War Time, subtract 4 hours; Central War Time, 5 hours, etc. The information is taken from the *American Ephemeris and Nautical Almanac.*

Sun. The sun will move eastward from $2^h\,32^m$ to $4^h\,31^m$, and northward from $+14°\,58'$ to $+21°\,52'$ during this period. Its path takes it from Aries into Taurus. At the close of the month it will be about five degrees north of Aldebaran, which bright star will necessarily be invisible for the period.

Moon. The phases of the moon will occur as follows:

	d	h	m
Full Moon	May 8	7	28
Last Quarter	15	11	12
New Moon	22	6	12
First Quarter	30	0	6

The moon will be nearest the earth on May 18, and farthest from the earth on May 2 and again on May 30.

Mercury. Mercury will be quite close to the sun during the early part of this period being at inferior conjunction on May 2. Towards the end of the month it will become visible in the east just before sunrise. It will be at greatest elongation west on May 29. It will, however, be nearly ten degrees south of the sun, which will make its visibility less favorable for northern observers.

Venus. Venus will move eastward at nearly the same rate as the sun, remaining less than an hour west of the sun. It will, therefore, be not at all conspicuous, in fact not visible without instrumental aid.

Mars. Mars will be moving eastward more slowly than the sun. It will be four or five hours east and a few degrees north of the sun. It will, therefore, be visible in the evening sky after sunset, a short distance east of Castor and Pollux, the Twins.

Jupiter. Jupiter will be visible in the west during the first half of the night during May. It will be in the constellation Leo, not very far from the bright star Regulus. It will be in quadrature, ninety degrees east of the sun on May 8.

Saturn. Saturn will be visible near the western horizon after sunset a few degrees north of the sunset point in the early part of the month. Later it will be lost in the evening twilight. It happens that at the middle of the month the sun, Saturn, Mars, Jupiter, and Neptune in that order, will be arranged in a series at approximately two hour intervals.

Uranus. Uranus will be too close to the sun for observation during May. It will be in conjunction with the sun on May 30.

Neptune. Neptune will cross the meridian just before midnight throughout the month, and hence will be well located for observation. It will be in the constellation Virgo, very close to the autumnal equinox.

Occultation Predictions for May, 1944

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the

signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

(Taken from the *American Ephemeris*)

Date 1944	Star	Mag.	IMMERSION Greenwich C.T.	*a*	*b*	Angle E from N	EMERSION Greenwich C.T.	*a*	*b*	Angle E from N
			h m	m	m	°	h m	m	m	°
Occultations Visible in Longitude +72° 30′, Latitude +42° 30′										
May 3	308 B.Leon	5.9	6 55.1	—0.2	—1.0	74	7 41.6	+0.3	—2.2	330
12	30 Sgtr	6.2	7 17.9	—1.9	+0.4	93	8 40.6	—2.0	—0.2	269
30	*l* Leon	5.3	0 39.2	—1.8	—1.0	97	1 54.8	—0.8	—2.5	320
Occultations Visible in Longitude +91° 0′, Latitude +40° 0′										
May 3	308 B.Leon	5.9	6 49.3	—0.7	—1.4	92	7 50.0	0.0	—2.2	316
4	*b* Virg	5.2	7 25.1	—0.5	—2.2	137	8 26.3	—0.4	—1.6	273
12	30 Sgtr	6.2	6 47.9	—1.2	+0.5	111	8 3.2	—1.9	+0.8	261
15	δ Capr	3.0	. ..	..	..	...	7 45.0	—0.3	0.0	312
30	*l* Leon	5.3	0 7.3	—1.8	—1.5	130	1 35.6	—2.0	—1.4	288
Occultations Visible in Longitude +120° 0′, Latitude +36° 0′										
May 3	308 B.Leon	5.9	6 24.8	—1.2	—2.2	136	7 42.0	—1.4	—1.7	283
4	*b* Virg	5.2	7 28.6	..	..	194	7 51.7	..	..	229
6	80 Virg	5.8	9 29.1	—1.4	—1.5	110	10 44.0	—0.9	—2.0	300
9	49 Libr	5.5	6 1.0	..	..	56	6 40.9	..	..	353
10	81 B.Ophi	6.3	7 12.4	—2.2	+1.8	71	8 15.2	—1.0	—1.2	328
12	30 Sgtr	6.2	6 42.9	..	..	172	7 0.9	..	..	205
14	20 Capr	6.2	11 39.6	—2.0	+0.7	90	13 0.2	—2.0	+0.9	242
16	74 Aqar	5.9	11 34.1	—1.2	+1.0	98	12 37.3	—1.3	+2.0	221
17	376 B.Aqar	6.3	12 11.5	—0.6	+3.2	11	12 50.9	—1.6	—0.1	302
29	34 Leon	6.4	6 1.8	—0.7	—1.3	94	7 3.2	+0.1	—2.1	313
Occultations Visible in Longitude +98° 0′, Latitude +30° 0′*										
May 3	308 B.Leon	5.9	7 0.3	—0.7	—1.9	120	8 7.8	—0.3	—1.6	292
4	*b* Virg	5.2	7 49.5	—0.1	—3.4	167	8 32.1	—0.8	—0.2	245
6	80 Virg	5.8	10 1.1	—0.6	—1.7	113	11 3.8	—0.1	—1.4	287
10	81 B.Ophi	6.3	8 5.1	—3.5	+2.1	52	9 4.3	—2.0	—2.8	333
12	30 Sgtr	6.2	6 44.3	—0.3	—1.4	149	7 32.4	—2.6	+2.6	227
15	δ Capr	3.0	. ..	..	..	...	7 38.2	—0.3	+0.8	278
30	*l* Leon	5.3	0 27.7	+0.1	+5.7	180	1 17.1	—4.7	+2.4	241

*Computation by Tecla Combariati and J. Lynn Smith, communicated by Captain J. F. Hellweg, Superintendent U.S. Naval Observatory.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

These Notes will consist of solutions or attempted solutions of the paths of several fireballs, the latter included because they were outstanding objects and because even yet these accounts may meet the eye of someone who never took the trouble to send in observations. Such a case recently happened and, on the basis of the new data, I am planning to see if solutions for heights cannot be made.

FIREBALL OF 1933 JULY 25/26

At 10:21 P.M., C.S.T., on this date a fireball was reported from two stations. S1, South Bend, Ind., $\lambda = 86° 15'$ W, $\phi = 41° 40'$ N, A. T. Keppler and V. P. Schlarb observers . . . "an exceptionally bright meteor . . . first seen in vicinity of Polaris and vanishing approximate $h = 15°$. Its flight was at an angle of 10° to the perpendicular and about 30° in length. The duration . . . nearly 3 seconds. Time 10:22 C.S.T. Color—white to brilliant green, magnitude —2.5." S2, Milwaukee, Wis., $\lambda = 87° 53'$ W, $\phi = 43° 00'$ N, C. C. Stevens and five other members of the Milwaukee Astronomical Society observers; 10:20 C.S.T., $a_1 =$ ENE, $a_2 =$ NExE, $h_1 = 18°$, $h_2 = 8°$. Duration of flight 3 seconds. Magn. = Venus, exploded, green. Also the path among the stars was given so we could check the coordinates.

It seemed there could be no doubt that these referred to the same fireball, as all details were similar. Yet when the azimuths were plotted and the heights solved for, I get $H_1 = 116 \pm 63$ km and $H_2 = 59 \pm 22$ km, both values reasonable enough but the deviations enormous! Hence, despite the seeming excellence of the observations, it is useless to go further except to say that the radiant was in $a = 37° \pm$. Incidentally S1 could have its a_2 interpreted as $170° \pm$ instead of $190° \pm$, but this would make the solution far worse than it is. The fireball's path was over central Michigan.

FIREBALL OF 1933 SEPTEMBER 12/13

At about 4:30 P.M., C.S.T., a brilliant fireball was seen from six places in Tennessee. Unfortunately, the data do not permit a solution, but a few facts can be stated. The path was undoubtedly east of $\lambda = 83°$ W and its motion towards the south; almost certainly it started south of $\phi = 37°$ N and probably ended near $\phi = 35°$ N. The slope was considerable, no less than 30°. No sounds were reported, nor any color or estimate of brightness. No train was left as is positively stated by the best observer. As it was seen by him from a distance of over 200 miles to the west, it must have been a conspicuous object to be noted in full daylight. Had it come after dark, its passage would have attracted wide attention. The data were plotted and studied by Miss Edith F. Reilly, then checked by me. I fully concurred in her conclusions.

FIREBALL OF 1933 SEPTEMBER 15/16

On this date at 8:58½ ± 3½ P.M., C.S.T., a splendid bolide was observed from two ground stations and from one airplane. The observations allow a consistent solution. S1, W. C. Johnson, Paducah, Ky., $\lambda = 88° 41'$ W, $\phi = 37° 07'$ N, $a_1 = 223°$, $h_1 = 16°$, $a_2 = 216°$, $h_2 = 2°$ (determined from a path plotted on a small-scale star map). Extremely bright and large, blue-green. S2, O. W. McCarty, Louisville, Ky., $\lambda = 85° 45'$ W, $\phi = 38° 15'$ N, $a_1 = 144°$, $h_1 = 40°$, $a_2 = 144°$, $h_2 = 5° \pm$ (determined from a good description of the path among the constellations). Greenish color, 1/5 diameter of Moon, 3 seconds duration. S3, article in "Courier-Journal" of Louisville, dated St. Louis Sept. 16 (A.P.) quoting from H. H. Gallup, who was flying a mail plane about 30 miles S.W. of Indianapolis from Columbus to St. Louis. ". . . suddenly flooded with light . . . the meteor rifled past the plane on the right. I would judge the distance at about 300 yards. The ship was going 150 miles per hour, the meteor . . . better than 500. The central body seemed to be about the size of a man's head, and there was a tail of fire several feet long . . . greenish-white light, much

more powerful than a landing flare or flood-light. My altimeter read 2000 feet . . . (flying) level. When the light of the meteor first hit the plane, the angle was such as to suggest the meteor was above and behind me, but it was going towards the earth at an angle. When the meteor was about 1000 yards ahead of the plane and 300 feet below its level there was a blinding explosion. I heard no noise nor did I feel the concussion which usually follows . . ."

With regard to the last report, it can only be taken as an excellent description without assuming the correctness of the estimated distances which are usually most deceptive. Even his "about 30 miles S.W. of Indianapolis" could hardly have been more than a rough estimate. That he heard no explosion though thinking it was so near is a fair proof that, as usual, it was much further off than supposed. However, from my plot his plane would be only 20± miles distant from the calculated end point, quite close enough to give all the phenomena he described. It is true that the end altitude from S1 is so very small that a fraction of a degree would have importance. From S2, the statement "below the trees" was taken to mean $h_s = 5° \pm$, therefore the end point may really be lower than is here given, even though this comes much lower than usual. The data on the path follow:

Date	1933 Sept. 15/16, 8:58½ ± 3½, P.M., C.S.T.
Sidereal time at end point	312°.6
Began over	$\lambda = 86° 26'$ W, $\phi = 39° 00'$ N at 86 ± 1 km
Ended over	$\lambda = 86° 41'$ W, $\phi = 39° 16'$ N at 16 ± 1 km
Length of path	82 km
Projected length of path	44 km
Observed velocity	27 km/sec (uncertain, as usual!)
Radiant (uncorrected)	$a = 324°$, $h = 57°.3$; $\alpha = 331°$, $\delta = +11°$
Zenith attraction	−2°.8
Radiant (corrected)	$a = 324°$, $h = 54°.5$; $\alpha = 333°$, $\delta = +9°$

From such a low ending point there would seem to be a good chance that meteorites fell. If so, they should be in the western part of Decatur Co., Indiana, near the village of Foresthill.

Flower Observatory of the University of Pennsylvania, Upper Darby, Pennsylvania, 1944 April 4.

Contributions of the Society for Research on Meteorites

Edited by **FREDERICK C. LEONARD**
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On the Origin of the Tektites

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(on leave)*

ABSTRACT

The conclusion that tektite falls were restricted in time to an earlier era

*[With the coming of the war, Dr. La Paz, the President of the Society for Research on Meteorites, suspended most of his investigations in meteoritics. Since January 1, 1943, he has been serving as Research Mathematician for the Office of Scientific Research and Development and, in addition, since January 3, 1944, as Operations Analyst for the Army Air Force.—ED.]

in Earth history is supported by all known facts, in particular, by the results of critical examinations of various reported falls, including 2 australite falls described not long ago by the late Dr. E. S. Simpson. On the contrary, if the tektites originate as "lunar impactites," as Dr. H. H. Nininger has recently suggested, it seems evident that they would have continued to fall onto the Earth right up to the present time. Furthermore, the probability that the distribution of "lunar impactite" falls over the Earth's surface would exhibit the alinement along 3 great circles, which is characteristic of all known tektite deposits, is vanishingly small.

It is frequently asserted that no tektites have been observed to fall, and the inference is then drawn that these bodies are not meteorites. It is true that meteoritical literature contains references to several observed falls of glass meteorites, but the evidence presented can be regarded as hardly conclusive.[1] Two of the most recent accounts of observed tektite falls are those given by the late Dr. E. S. Simpson in the *Journal of the Royal Society of Western Australia,* **21**, 1934-35 (the Lake Grace fall), and **25**, 1938-39 (the Cottesloe fall). Altho considerable circumstantial detail is given for both falls, the writer was led to doubt the authenticity of the Lake Grace fall because of the excessive depth (30 cm.) to which the 31-gram australite is reported to have penetrated the plowed ground. An application of the same sort of criterion, earlier used to verify that the Norfork, Arkansas, iron could not by itself have attained the great depth at which it was found in the Earth,[2] gave what was regarded as conclusive proof that a body with the low density of the Lake Grace australite could not have penetrated 30 cm. into the ground. On the contrary, the evidence regarding the Cottesloe fall seemed so convincing that I was led to write, first to Dr. Simpson, and then, after his decease, to Dr. H. Bowley, Government Mineralogist for Western Australia, in the hope of securing sufficiently detailed information in regard to the Cottesloe fall to be able to compute an orbit for the australite in question. A reply to my last inquiry has just been received from Dr. Bowley; it contains a copy of a note prepared by him for the Royal Society of Western Australia, from which I quote the following interesting passages:

"In the *Royal Society Journal,* **25**, 1938-39, the late Dr. E. S. Simpson described an australite which a Mr. F. Hanson stated he had seen to fall in Kathleen Street, North Cottesloe.

"Following an inquiry from the Society for Research on Meteorites in America, I wrote to Mr. Hanson, asking for the time and date when he saw this australite fall. A Mr. Hammer, a relation of the Hanson family, came into the Laboratory in answer to this letter. He stated that the australite, shown to Dr. Simpson, and now in the Western Australian Museum, is one that he found in a ballast pit on the railway between Narrogin and Merredin, in 1925, and not the one stated to have been seen to fall by Hanson in 1935. This australite was found about 3 feet, 6 inches below the surface, while working the ballast pit; the australite was covered in clay and had been there evidently a long time. The men working on the railway chipped the australite by placing it on the rail near where it was found and trying to break it with a hammer or pick. Hammer gave this australite to the Hanson family.

"The object seen to fall by Hanson is now lost. It was stated by Hammer that it was irregular in shape and about the size of a hand. Hanson told Hammer that it made a small pit in the tennis court where it fell; there was a flash of light and a smell, and the object was warm when dug up.

"Subsequently, Mr. J. C. Hood, thru whose friendship with Mr. A. N. Carter, Hanson's employer, the australite had first been brought to Dr. Simpson's notice, saw Mr. Carter, who questioned Hanson very carefully again. Hanson admitted that the australite given to Dr. Simpson and stated to be the one seen to fall was actually the australite given to the Hanson family by Hammer many years before. Hanson stated that he did not know that there were 2 australites in his house at the time he was asked to bring in his find to show Dr. Simpson.

"Until the object seen to fall at Cottesloe can be found, it is not known whether it is an australite or not. The australite at the W. A. Museum did not come from Cottesloe, but from a spot between Narrogin and Merredin, the exact locality of which has not yet been ascertained."

On the basis of the facts unearthed by Dr. Bowley and my calculations concerning the probable maximum depth to which the Lake Grace australite could have penetrated, it would appear that neither one of the falls reported by Dr. E. S. Simpson may have been genuine. It happens that the same conclusion has been reached by most of the authorities who have investigated the other falls described in reference (1). In my opinion, even the once-famous Igastite fall, described by Brezina and Meunier, was not genuine, but is to be explained like the Zenith, Kansas, stone—accepted as meteoritic by Nininger and Muilenburg[3]—as the highly siliceous residue produced by the burning of a stack of compressed straw.[4]

It appears, therefore, that no incontestable case of an *observed* glassy-meteorite fall has as yet been recorded. Quite independent of the evidence of this absence of observed falls, the conclusion that all glass meteorites fell onto the Earth long ago has been arrived at from other directions. Thus, in a recent letter to the writer from Dr. C. Fenner, one of the foremost authorities on the australites, it is stated that the idea of continuing falls of these bodies appears to be quite preposterous, in view of ". . . the uniformity of types, uniformity of compositions, and uniformity of area affected."

The evidence available strongly suggests, therefore, that the occurrence of such falls of tektites as the australites was restricted to a much earlier era in Earth history.[5] On the contrary, if the tektites originate as "lunar impactites," as Dr. H. H. Nininger has recently suggested,[6] it is difficult to see why they have not continued to fall onto the Earth right up to the present time. Nininger, in developing his conjecture as to the origin of the tektites, makes the quite indefensible assumption that the Moon is totally devoid of an atmosphere[7] and paints a vivid picture of the explosion resulting from the direct impact of a meteorite upon the unprotected lunar surface, which ". . . sends a violent charge of fragmental lunite in all directions." From a knowledge of the number and the size of the meteorites annually reaching the Earth's surface, of the orbital velocities of these bodies, and of the loss of meteoritic mass caused by ablation,[8] during passage thru the atmosphere, it can be inferred with certainty that a very large number of meteorites weighing at least 10 pounds and having speeds of from 10 to 40 or more miles per second plunge into the atmosphere of the Earth in the course of a year's time. We shall refer to these meteorites as belonging to the class *M*. Now it has been known for some 80 years that the gravitational attraction of the Earth does not materially increase the number of meteorites which it picks up in its motion around the Sun.[9] Hence, we may conclude from the available evidence that the *space density* of meteorites of the class *M*, in the region traversed by the orbit of the Earth, is itself fairly high. Conse-

quently, one has to recall only that the true orbit of the Moon in space is almost identical with that of the Earth[10] in order to appreciate that our next-door neighbor must be subject to almost the same bombardment from meteorites of the class *M*, as is the Earth. Therefore, if the Moon were devoid of an atmosphere, as Nininger supposes, one cannot escape the conclusion that its unprotected surface would be struck annually by a large number of meteorites weighing 10 pounds or more and moving with speeds greater than 10 miles per second. Each such meteorite encountered by the Moon, after forcibly penetrating the selene surface to a certain depth by a process resembling the action of a punch in a hyper-speed punch-press, would explode with a violence far exceeding that of any know explosive. Furthermore, the debris thrown out by such meteoritic bombardment would be given *initial* velocities ranging from somewhat less to considerably more than the velocity of escape from the Moon, for *static fragmentation* tests have shown that even the ordinary military explosives used as bomb fillers impart *initial* velocities exceeding 8000 feet per second[11] to bomb-case fragments. On the Earth, air resistance swiftly reduces such extreme velocities, but on an airless Moon, lunitic debris given initial velocities, however large, would suffer no deceleration from resistance effects and would therefore either move away from the Moon in elliptic orbits of very large semi-major axes or escape entirely from the gravitational control of that body. In either case, many of the possible directions of departure of impactites would determine trajectories intersecting the Earth, and this statement is valid no matter where on the Moon's surface the point of impact is situated. In fact, examination of such *orbits of ejection* as the dotted curve shown in Fig. 17 on p. 463 of Dr. F. R. Moulton's monumental treatise on *Periodic Orbits* (1920) will convince one that even in the extreme case where the meteoritic impact and, therefore, the ejection of impactites occur at the point on the Moon *antipodal* to the Earth, it is possible to assign in many ways initial velocities and directions of departure such that the corresponding trajectories intersect the Earth.[12]

We are, therefore, forced to conclude that, under Nininger's hypotheses, even the *annual* crop of lunar impactites destined to fall onto the Earth would be a bountiful one,[13] and, moreover, would be distributed more or less at random all over the surface of our globe. Futhermore, it is evident that weathering cannot be invoked to provide for the rapid disintegration and disappearance of such highly resistant glass bodies, once they have arrived on the Earth, as has been done in the case of certain friable aerolites. Under these circumstances, we must leave to the author of the lunar impactite theory the task of explaining why there is no evidence on the Earth of the fall of tektites since the Pleistocene epoch and why all tektite deposits known at present have been found on or near 3 great-circle arcs along which, significantly enough, are alined also most of the accepted meteorite craters and many other evidences of meteoritic impact.[14]

NOTES AND REFERENCES

[1] See, *e.g.*, C. Grewingk and C. Schmidt, *Archiv f. Naturk., Liv.-Esth.-und Kurlands*, **3**, 421, 1864, and *Neues Jahrb. f. Min.*, **1**, 399, 1905; G. Brandes, *Zeitschr. f. Naturwiss.*, **76**, 459, 1903, and *Neues Jahrb. f. Min.*, **1**, 398, 1905; A. Brezina, *Anzeiger Wiener Akad. Math.-Naturw. Kl.*, Feb. 11, 1904.

[2] L. La Paz, *C.S.R.M.*, **2**, 58; *P. A.*, **46**, 523, 1938.

[3] On the Zenith "fall," see *Am. Jour. Sci.*, **26**, 495, 1926; *Jour. Geol.*, **39**, 596, 1931, and H. H. Nininger's *Our Stone-Pelted Planet*, 63, 1933.

[4] This explanation of the origin of the peculiar siliceous "meteorites," occasionally reported to have fallen in Kansas, South Dakota, and other wheat-

growing States, dates from my investigation of the Wichita, Kansas, "fall" of March, 1922. Many apparently reliable persons testified to the authenticity of this "fall," and thin sections of the "meteorite," prepared by Ward's Natural Science Establishment and examined in the Laboratories of Mineralogy and Petrography of Harvard University, by Dr. John E. Wolff, were found to resemble those from the Allegan, Michigan, stone described by G. P. Merrill. Nevertheless, study of the residues left by the burning of old, compacted straw-stacks and piles of baled straw convinced me that the Wichita specimen originated in such a fire. Initially, Merrill favored the idea that the Wichita specimen was ". . . an *artificial*, rather than a natural, fulgurite, that is, [that it] may have been produced by some high-voltage current from a broken conduit." However, in the last letter I had from him on the subject (written under date of October 5, 1925), he accepted the view that the Wichita specimen was the residue from a burned straw-stack.

[5] It is necessary here to distinguish clearly between tektites and impactites like the Wabar and Darwin glasses. Such silica glasses may be formed at any time by the heat incident to high-speed meteoritic impacts on the Earth's surface.

[6] H. H. Nininger, *Sky and Telescope*, **2**, No. 4, 12, and No. 5, 8, 1943.

[7] On the subject of the atmosphere of the Moon, see L. La Paz, *C.S.R.M.*, **2**, 35; *P. A.*, **46**, 277, 1938. A good deal of the material in a summary recently published by Dr. Roy K. Marshall in *P. A.*, **51**, 415, 1943, is taken from this 1938 *C.S.R.M.* paper.

[8] L. La Paz, *Astr. Nach.*, **267**, 110, 1938. In view of the terminology employed by Nininger (*loc. cit.*, ref. ([6]), *ante*, No. 4, 14) it appears desirable to point out that the meteoriticist uses the word *ablation* to describe the surface wastage of a meteorite occasioned by melting and evaporation during the passage of the meteorite thru the atmosphere. This usage has long been well established in other analogous connections; *cf.* R. S. Tarr, *College Physiography*, 205, 1918; A. W. Grabau, *A Comprehensive Geology*, **1**, 391, 1920; and F. H. Lahee, *Field Geology*, 355, 1941.

[9] H. A. Newton, *Am. Jour. Sci.*, Ser. 2, **39**, 199, 1865. See also C. P. Olivier, *Meteors*, 52, 1925.

[10] The path of the Moon in space deviates only very slightly indeed from the orbit of the Earth. According to Russell, Dugan, and Stewart, *Astronomy*, **1**, 165, 1926, if the orbit of the Earth is represented by a circle with a radius of 100 inches, then the space path of the Moon never departs from this circle by more than ¼ of an inch! *Cf.* also the drawing of the space paths of the Earth and the Moon given by D. P. Todd in *A New Astronomy*, 232, 1897.

[11] The parabolic velocity in the case of the Moon is slightly less than 8000 feet per second; see Russell, Dugan, and Stewart, *Astronomy*, **1**, 271, 1926.

[12] The figure given by Moulton is drawn for the case in which the infinitesimal body is *ejected* from the point on the Earth $(1-\mu)$ antipodal to the Moon (μ) and eventually passes close to our satellite, the mass, μ, of which is relatively insignificant. However, it is evident that by interchanging the roles of $1-\mu$ and μ (in which case the orbit of the infinitesimal body passes close to the much more massive Earth) and by suitably choosing the initial conditions under which ejection from the Moon occurs, orbits actually intersecting the Earth can be obtained.

[13] Nininger, on the contrary, believes that only in highly exceptional cases would lunar impactites fall onto the Earth. He is led to this conclusion by greatly overestimating the extent to which the Earth serves to shield the Moon from meteoritic bombardment and by adopting the erroneous view that only meteorites ". . . landing near the perimeter of the Moon's face . . ." can send a spray of impactites toward the Earth.

[14] In regard to the observed distribution of the tektites along great-circle arcs, see L. La Paz, *C.S.R.M.*, **2**, 28; *P. A.*, **46**, 224, 1938. All known tektite deposits occur on or near either the 2 great circles discussed in that paper or a third great circle passing thru the place of find of the *bediasites*. In the writer's opinion, the observed distribution along 3 great circles, of all of the many known tektite deposits, containing millions of individual specimens, is a fact of

the utmost significance. No one who ignores this fact, as Nininger does, in the paper cited in ref. (⁶), *ante*, can possibly reach a correct conclusion as to the origin of these bodies. It is therefore a matter for concern that geologists and others have given quite misleading accounts of what has been written concerning this singular distribution of the tektites. Anyone who takes the trouble to read the original paper of David, Summers, and Ampt on tektite distribution (*Proc. Roy. Soc. Victoria*, **39**, *N. S.*, Pt. II, 167, 1927) will discover that there is not the slightest foundation for the statement made by V. E. Barnes (*Univ. of Texas Publ. No. 3945*, 491, 1939) to the effect that David *et al.* postulated that all tektites ". . . are located on one great circle around the Earth." What David and his associates actually did was to point out the *observed fact* that all deposits of tektites known *at the time they wrote* (1927) lay on or near a single great-circle arc. Similarly, the present writer drew attention in his 1938 paper (*loc. cit., ante*) to the *observed fact* that, *at the time he wrote*, all known tektite deposits lay on or near 2 great circles. He also suggested one possible interpretation of this observed fact which would provide an explanation of the failure of all attempts made *up to that time* to discover tektites in the United States, but he did *not* assert, as Nininger has contended (*loc. cit.*, ref. (⁶), *ante*, No. 5, 9), that ". . . no tektites would ever be found in North America because this continent lay outside the great circles."

Various critics have attempted to discredit the writer's 1938 *C.S.R.M.* paper on the tektites because the discovery of the bediasites in Texas, in 1936, necessitated the introduction of a *third* great-circle locus. Such attempts disclose only a lack of appreciation of the true issues involved. The surprising thing is not that 1 or 2 great circles did not suffice to pass thru all tektite deposits but rather that *some small number* of great circles was sufficient. As long as millions of tektites which *might* have shown a *random* distribution are known to exhibit alinement along a *few* great circles, we are justified in demanding that this *observed fact* be explained by any theory which is advanced to account for the origin of the tektites. As the writer has shown in his 1938 *C.S.R.M.* paper, the probability that the curious distribution exhibited by the tektites is the result of mere chance is vanishingly small.

Meteoritical Literature Recently Received

(1) *Meteoritica:* published mostly in Russian, under the auspices of the Meteorite Committee, and by the Publishing House, of the U.S.S.R. Academy of Sciences, Moscow and Leningrad, 1941: Dr. W. I. Vernadsky, Member of the Academy, Editor: Vol. 1, 124 pp.; Vol. 2, 132 pp., each volume including numerous illustrations (plates *&c.*). A little of the material, fortunately for readers who are not acquainted with the Russian language, is published likewise in English; that not more, or all, of it appears in English, as well as in Russian, is to be regretted. It is a pleasure, however, to welcome this new publication, or periodical, in the field of meteoritics and to note the evidence that it contains of the activities of meteoriticists in the U.S.S.R.

(2) "The Physical Characteristics of Meteors," by R. A. McIntosh, F.R.A.S., of the New Zealand Astronomical Society, *Trans. Roy. Soc. New Zealand*, **72**, Pt. 4, 301-10, March, 1943.

(3) "Mysterious Craters of the Carolina Coast: A Study in Methods of Research," a "Sigma Xi National Lecture," by Douglas Johnson, Professor of Geology, Columbia University, New York City, *Am. Scientist* (the quarterly publication of the Society of the Sigma Xi), **32**, No. 1, 1-22, 8 figs. and a frontispiece of the author, January, 1944. (The death of Professor Johnson, on February 24, 1944, is reported in *Sci., N.S.*, **99**, 174, 1944.)

Erratum in the March Issue

In the "Classification of Meteorites," on p. 149, under "Siderites (Si)," "Nickel-poor ataxites" should read "Nickel-poor ataxites (D_3) and."

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VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

The Eclipsing Variable SX Cassiopeiae: In a recent article in the *Astrophysical Journal,* Dr. Struve, from a study of the spectrum plates taken in 1943 at the McDonald Observatory, concludes that there are streams of gas spreading out between and around the two components of the eclipsing variable SX Cassiopeiae —somewhat analogous to the condition which is found to exist in the system of β Lyrae—and that the remarkable variations found to prevail in the velocity-curve, discovered by Joy at Mount Wilson, are probably caused by changes in the density of the gas within the streams. Struve also thinks that the large scatter found to exist in Dugan's photometric observations of this same star may be attributed to this same condition.

The elements derived from the velocity-curve are decidedly at variance with those found from Dugan's photometric light curve,—observed on 62 nights,— and Struve proceeds to explain this discrepancy as caused by the presence of the above-mentioned streams of gas between the components.

SX Cassiopeiae was discovered photographically by Madame Ceraski in 1907. Numerous visual light curves were derived, but the most reliable and extensive of them is doubtless that by Dugan who found the period, $36^d.56757$, to be essentially constant and the secondary minimum occurring half-way between the principal minima. The visual curve has a principal minimum depth of 0.99 mag. and the secondary, of 0.32 mag. The eclipse is total and lasts for 3.74 days, with an eccentricity of 0.0. Dugan was of the opinion that the large amount of scatter in his observations is real and attributed it to the variations of the brighter A star.

Joy found that at primary minimum only the G6 component was visible, with no trace of the A6 star, for one day on each side of mid-eclipse. Up to about two days before and after mid-eclipse the two spectra, A6 and G6, are blended. At other times Joy found no indications of the G6 spectrum.

The McDonald spectra cover parts of three consecutive cycles, one very completely observed. The radial-velocity observations, when plotted, show an unsymmetrical maximum at phase 32 days, and a broad flat minimum near phase 14 days. The most striking feature of the elements derived from this velocity-curve is the value of 0.5 for the eccentricity, as against 0.0 from Dugan's photometric curve.

When the observed velocity-curve is compared with the curve computed on the assumption that $\gamma = -8$ km/sec and $k = 27$ km/sec, it is noted that the negative departures can be attributed to an expanding stream of gas from the A star, as seen between the 10-day and 27-day phases; while the positive departures between 30 and 34 days are attributed to a cool receding stream of gas flowing from the G star toward the following side of the A star.

Recently, Dr. S. Gaposchkin has compared with Dugan's visual light curve two independent photographic light curves of this star. One was derived from Harvard plates by Gerasimovic from observations made by Mrs. Gerasimovic, the other was derived by Drs. R. Prager and S. Gaposchkin from observations made by Miss Brenton. The two photographic curves are identical. It is found that the photographic curve differs radically from the visual curve, principally in the duration of eclipse and of totality.

	Visual (Dugan)	Photographic (Gaposchkin)
Mag. Max.	8.55	...
Mag. Min.$_1$	9.54	...
Mag. Min.$_2$	8.87	...
Duration of Eclipse	$3^d.74$	$2^d.74$
Duration of Totality	$1^d.3$	$1^d.8$
Time of Min.$_2$	$0^P.5$	$0^P.5$

The difference in magnitude, and in depths of minima is consistent with the difference in the spectra of the two components, but the difference between the duration of eclipse, as well as the steepness of the minima, can be explained, according to Gaposchkin, only in terms of the different apparent radii of the two components as derived from the two light curves:

	Visual (Dugan)	Photographic (Gaposchkin)
Radius of A component	0.12 A.U. = 11 ☉	0.04 A.U. = 3.5 ☉
Radius of G component	0.25 A.U. = 22 ☉	0.20 A.U. = 18 ☉

Gaposchkin's figures indicate that the radii of the components deduced from the two solutions are not the same, as has always been assumed heretofore in dealing with eclipsing stars. The deduced difference of the radii confirms the existence of the gaseous envelope detected by Struve. The masses can be assumed to be constant and therefore, depending on which curve is used, photographic or visual, the density decreases from 0.80 ☉ to 0.05 ☉, a ratio of 1 to 16.

Extrapolating the observations in photographic and visual light to the infrared, and assuming that the relation is linear with wave-length, it is found that at 10,000 Å the stars should be in contact. In other words all the space between the components must be filled with gas.

Current Notes on A.A.V.S.O. Variables: 005060, γ Cassiopeiae has recently increased in brightness from magnitude 2.9 about January 1, to 2.7 on March 1.

020356, *UV Persei* was at maximum on January 1, 1944, magnitude 12.0.

054319, *SU Tauri* was reported as below normal maximum brightness late in February. On March 10 the magnitude was observed at 11.5, nearly two magnitudes fainter than normal. The previous drop to minimum occurred in April, 1942, when the variable decreased in light to fainter than magnitude 13.0. The star attained full brightness only last November. This region is well placed for observation for another month or two and accordingly a close watch should be kept on the variable, not only visually but spectroscopically. Minimum light has been observed as faint as magnitude 16.0.

060547, *SS Aurigae* was observed at maximum early in February; "long" type, magnitude 9.5. A "narrow" type maximum was observed early in December, 1943, preceded by a "long" type maximum in September, 1943.

074922, *U Geminorum* should be closely watched for another rise to maxi-

mum. The last previously reported observed maximum occurred in September, 1943.

080362, *SU Ursae Majoris* was observed at maximum in January, 1944.

080835, *Nova Puppis* is slowly fading in light. An excellent observing chart of this region, with magnitudes of comparison stars derived by Dr. Edison Pettit, can be found in the February issue of the *Publications of the Astronomical Society of the Pacific*. The magnitude of the nova, as reported in this same issue by Pettit, was 9.5 on January 8, 1944.

081473, *Z Camelopardalis* has passed through four well-observed maxima since October 1, 1943; about October 20, November 20, January 2, and February 6, respectively. The intervals between successive maxima during these months are somewhat longer than usual, between 20 and 34 days. There is an indication that a maximum might have occurred about December 11, 1943.

145971, *S Apodis*, according to observations made in South Africa in November, 1943, was still faint, magnitude 13.0, or slightly fainter.

154428, *R Coronae Borealis* is still at full maximum brightness, magnitude 6.0. This star has been at maximum since May, 1943. Its next minimum is, of course, unpredictable; it may occur any time within one to ten years.

180445, *Nova (DQ) Herculis* has decreased to below the twelfth magnitude in its downward course to pre-outburst condition.

191033, *RY Sagittarii* is again at normal maximum brightness, around the seventh magnitude.

194632, *χ Cygni* attained a maximum magnitude of 5.0, about the middle of January of this year. There was only a slight indication of a "stillstand" on its way to maximum, at about magnitude 7.5.

201520, *V Sagittae* has not been brighter than 12.0 since early in October, 1943. This is an exceedingly interesting and peculiar variable which has been considered by some investigators as an old nova.

210868, *T Cephei* was at maximum during December, 1943, magnitude 5.5. It is now well on its way to minimum light.

213843, *SS Cygni*, as mentioned in these "Notes" for March, was at maximum early in February; the "long" type. The next rise to maximum is difficult to predict; it may occur in April, or not until May, 1944, although recent cycles have been of the order of 90 to 100 days in length. Because of its unfavorable position in the sky, the star is best observed in the early morning hours for the next few months. Let us see that not another maximum passes unobserved as was the case in 1942, when the only unobserved maximum in over forty-five years occurred.

Observations: Summary of observations, as contributed during the month of February, 1944, by 39 observers, is given below.

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Boone	1	1	Halbach	27	27
Bouton	11	11	Harris	37	37
Buckstaff	10	16	Hartmann	76	76
Cousins	24	49	Houston	18	35
Fernald	211	376	Howarth	17	18
Ford	15	15	Irland	18	23
Garneau	21	21	Kearons	66	94

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Kelly	14	19	Peltier	140	193
de Kock	70	425	Robinson	37	71
Koons	47	48	Rosebrugh	23	144
Krumm	14	31	Schoenke	17	21
Lovinus	2	2	Segers	17	39
Luft	11	124	Sill	71	71
Mary	8	8	Topham	45	45
Maupomé	44	48	Vohman	19	19
Meek	34	155	Webb	12	16
Milne	3	3	Weber	66	66
Nadeau	99	133	Wolfram	2	2
Oheim	13	21	—		—
Oravec	8	15	39	Totals	2522
Parks	27	44			

March 21, 1944.

Comet Notes

By G. VAN BIESBROECK

There are no comets, bright enough to be seen in ordinary telescopes, known to be present in the sky at this date. Only two faint periodic comets are under observation.

Periodic Comet Comas Solá follows closely the ephemeris given on p. 154 of the March issue. It is gradually fading and becomes more diffuse. There is still a short tail opposite to the direction of the sun. The total brightness corresponded to a star of magnitude 14 as seen here last night in the evening sky. Visibility may continue for some time but the comet will run into daylight before another month.

Periodic Comet 1943 *a* (Oterma) has been recorded again at the end of last month. Its appearance as a fuzzy little coma remains unchanged around magnitude 16. In spite of its faintness this object deserves attention on account of its unusual orbit and great distance to which we called attention previously. Of course none but the larger telescopes will reach such a faint comet.

Next month observers will begin looking for the return of Periodic Comet Encke in the morning sky. It comes to perihelion in August but will be unfavorably situated for most of the present apparition. The following ephemeris, computed by M. Sumner, is from the *Handbook of the British Astronomical Association* for 1944.

1944	α h m	δ ° ′	Distance from Sun	Distance from Earth
May 2	1 34.0	+15 56	1.77	2.72
10	1 51.4	17 41	1.67	2.60
18	2 10.5	19 30	1.57	2.47
26	2 31.7	21 22	1.46	2.34
June 3	2 55.5	23 15	1.35	2.20
11	3 22.8	25 6	1.23	2.07
19	3 54.4	26 50	1.11	1.93
27	4 31.6	28 16	0.98	1.80
July 5	5 15.4	+29 7	0.85	1.68

McDonald Observatory, Fort Davis, Texas, April 12, 1944.

General Notes

Dr. Otto Struve, director of the Yerkes and the McDonald Observatories, has been awarded the Gold Medal of the Royal Astronomical Society for his work on the observation and interpretation of the spectra of stars and nebulae. (*Nature*, March 18, 1944)

Astronomical Paintings

The University of Minnesota presents during April certain astronomical paintings by Leslie Lavelle, Minnesota painter. Miss Lavelle's paintings are the result of four years of study and research. The character of the constellations is maintained in abstract design, based on the factual precision and geometry of the stars, and the color of the brightest star in each group determines the color motif. Color brilliance and dramatic design make the paintings unique adventures on celestial frontiers.

The Herschel Astronomical Club of Fall River, Massachusetts

The Herschel Astronomical Club met on the evening of March 7, 1944, to mark the departure of two of our most active and important members, the Reverend and Mrs. William M. Kearons.

Ill health has caused Mr. Kearons to resign his duties in Fall River, and his absence will be felt, not only by the members of our society, but by a great many others in the community who found him an untiring worker in many fields, notably Astronomy and Celestial Photography. Mrs. Kearons has kept an accurate record of sun-spot activity for many years, and is well known for her work in Variable Star observing.

At present, our members are aiding in the rehabilitation of a fine Alvin Clark eight-inch telescope, with mounting by Warner & Swasey, which was installed nearly a half century ago in the local High School.

FRANK J. KELLY, *Secretary.*

The Royal Observatory

The announcement in the daily Press that the Admiralty has decided, in principle, that the Royal Observatory shall be moved from Greenwich to a new site, where conditions are more favorable for astronomical observations, will not have come as a surprise to those who have watched the trend of events in recent years. Rather is it a matter for surprise that the Observatory has been able to carry on for so long, under conditions of increasing difficulty, on its original site. Many observatories elsewhere have been compelled by similar circumstances to move. In the case of the Royal Observatory, the long associations with Greenwich, the advantages in fundamental astronomy of continuity of observation on the same site and with the same instruments, and its position on the prime meridian, have no doubt all played a part in postponing a decision the ultimate inevitability of which must long have been apparent.

The Royal Observatory was founded in 1675 by Charles II, to meet the needs of navigation. The problem of finding longitude at sea had then become urgent. The positions of the moon and stars were not known with sufficient accuracy to enable the method of lunar distances to be used. The Royal Warrant for the

building of the Observatory states that "in order to the finding out of the longitude of places for perfecting navigation and astronomy, we have resolved to build a small Observatory within our park at Greenwich, upon the highest ground, at or near the place where the Castle stood." Sir Christopher Wren was appointed as architect and the Rev. John Flamsteed was appointed "our astronomical observator" and directed "to apply himself with the most exact care and diligence to the rectifying the tables of the motions of the heavens, and the places of the fixed stars, so as to find out the so-much-desired longitudes of places for the perfecting the art of navigation."

* * * * *

It is to be hoped that the original building, which Wren said that he built "a little for pompe," and the old buildings clustered around it will remain and that the historic instruments—including Halley's, Bradley's, and Pond's transits, Bradley's zenith sector, with which aberration and nutation were discovered, the old quadrants, and, most famous perhaps of them all, the Airy transit circle—will be suitably displayed in them. They will become an object of pilgrimage not merely to astronomers but also to many visitors to Great Britain from all parts of the world.

Much of the work of the Royal Observatory has had to be removed elsewhere during the War, either for reasons of security or because of enemy action. The continuity of many programmes of observation has been broken. It is to be hoped that a new home will be made available as soon as conditions permit, so that the various branches of the Observatory can be brought together again and normal work resumed under more favorable conditions. It is understood that Treasury sanction will be required; but surely in a case such as this, to obtain it will be little more than a formality. The new home should be one befitting the long history and great traditions of the Royal Observatory. What could be more appropriate than that one of our large historic country houses, mellowed with age and rich in associations with the past, should form the nucleus of the new Observatory, the various telescopes being erected in the surrounding grounds? A fundamental need is for a neighbourhood not likely to be affected by the spread of industry, and it should not be impossible to find such a site. (*Nature*, February 12, 1944.)

Library of Congress Exhibit Portrays Work of the Academy of Sciences of the U.S.S.R.

An exhibition portraying the history and activities during the last 25 years of the U.S.S.R. Academy of Sciences, the principal learned body in the Soviet Union, has recently been placed on display in the Library of Congress.

Founded by Peter the Great in 1724, the Academy today consists of approximately 136 academicians, more than 30 honorary academicians, about 224 corresponding members and over 5,000 scientific and technical assistants. Sixteen American scientists are now honorary or corresponding members of the Academy. The portraits of some of the more prominent academicians have been included in the Library's exhibition through the cooperation of the Embassy of the U.S.S.R. in Washington. Representative volumes of the more important works by members of the Academy have been selected for display from the extensive collection of Russian materials in the Library of Congress, probably the richest to be found in any library in the Western Hemisphere.

The peacetime work of the Academy was suddenly interrupted on June 22,

1941, when Germany invaded Russia. From the very beginning of the invasion, the Academy of Sciences readjusted its activities to place its resources fully behind the Russian war effort. Even while Mosoow was under heavy German attack, the institution continued the publication of its learned journals and texts. Books printed while the city was under Nazi bombardment are among those shown in the Library's display.

Under the Academy's direction, chemists have pioneered in manufacturing synthetic rubber, in photochemistry, in developing winter lubrications for tanks and planes, in producing new explosives and in extending the uses of helium. Soviet geologists have turned their energies to the problem of supplementing the stock of raw materials required by the Russian war machine, and agronomists have increased the productivity of agriculture. Physiologists and physicians of the U.S.S.R. have won international fame for their treatment of shock, tetanus, gangrene and other war maladies, and dietitians have found new nutritive substances, as well as new sources of vitamins, which have been used to help solve the food problems resulting from the war. Technologists have also scored notable successes in finding substitutes for scarce materials, in simplifying technological processes and perfecting the organization of war industries. Most of these activities are represented in one way or another by publications on display.

Exhibited items of particular interest include the first book published by the Academy, the "Commentarii academiae scientiarum imperialis petropolitanae," published in 1728 at St. Petersburg; pictures of the first building of the Academy in Leningrad, its present home in Moscow to which it moved in 1934, and the architect's drawing of its proposed new building; numerous publications of various scientific establishments attached to the General Assembly, and current periodicals concerning the Academy as a whole. It is interesting to note that, while the publications of the Academy are published mainly in Russian, a number have been published in English as well, while others have titles and summaries in English. M. V. Lomonosov (1711-1765), whose portrait appears in the historical section of the exhibit, is described as "probably the most interesting figure in the whole existence of the Academy."

Book Reviews

Basic Problems in Celestial Navigation—1944, by Barton & Roth, Hayden Planetarium. (Addison-Wesley Press, Inc., Cambridge 42, Mass. $1.00.)

Basic Problems in Celestial Navigation offers, for the first time, a selected sequence of problems which have been arranged from the point of view of developing, step by step, the many phases of the navigator's work.

Each new step has been introduced by an explanation of the terms involved and the procedure has been illustrated by one or more examples. When necessary, detailed "rules" have been inserted and clarified by specific applications. Conventional diagrams supplement many of the explanations of the more complex relationships and the frontispiece is unique in its usefulness.

The selection of the problems is outstanding for the variety of situations encountered—situations involving the Seven Seas, crossing the equator, the international date line, around the hours of the clock, and through the seasons of the year.

Work sheets, for which the simplicity of the form is at once evident, have been prepared for the solution of problems by the current methods of H.O. 208, H.O. 211, Ageton's Assumed Position Method, and H.O. 214.

Both the beginner and the more advanced student will welcome the book as an opportunity to acquire a comprehensive picture of the paper work of navigation. Experienced navigators should find the problems refreshing.

Handbook of Chemistry (Fifth Edition), by Norbert A. Lange. (Handbook Publishers, Inc., Sandusky, Ohio. $6.00.)

The fifth edition of this work is at hand. The third and fourth editions, respectively, were described in this magazine in 1939 and in 1941. There has been a notable growth in size in these three editions, from 1,850 pages to 1,985 pages to 2092 pages. This is the result of expansion of former tables and the addition of new ones. Six tables, Periodic Table (Deming), Flammable Liquids, Flame Temperatures, Plastics, Fluorescence of Chemicals, Minerals, and Gems, Water for Industrial Use, appear for the first time in this edition. The Table of Contents lists more than 170 different titles of tables. The most extensive table is the one giving the physical constants of organic compounds. It contains 6,507 entries from abietic acid to zingerone. One cannot begin to enumerate the great variety of data here presented. It would be difficult to think of any item in the category of tables that is lacking.

The last 271 pages are devoted to mathematical formulae and tables. The formulas range from the elementary algebraic relations to vector analysis. The tables include the usual 5-place logarithmic table of numbers and of trigonometric functions. Many other tables are given also. The index of 28 pages includes more than 3,600 entries, thus greatly facilitating the finding of any desired item.

This volume is a veritable library in itself and will be consulted many times by workers in the natural sciences.

Its form is adapted to such frequent use. The paper and binding are sturdy and will withstand much usage. It lies open easily at any page, which is often a great convenience. It can be heartily recommended for the purpose for which it is intended. C.H.G.

World Wide Planisphere, by Wm. H. Barton, Jr., Curator Hayden Planetarium. (Addison-Wesley Press, Inc., Cambridge 42, Mass. $2.50.)

The World Wide Planisphere is unique in its usefulness. The ordinary planisphere or star-finder is built for one latitude only, but this amazingly versatile map can be used in any latitude with great accuracy. It is the answer to the fervent prayer of both the navigator and the amateur astronomer. Easily used, sturdily constructed, it is the easiest of all devices for learning stars as seen from various latitudes.

Two basic maps are given for each hemisphere of the sky. One is especially designed for use by navigators, and gives the names of the navigation stars as listed in the American Almanacs, with helpful guide lines from one group to another. The other presents the constellations as the amateur astronomer would learn them.

Masks are provided as an integral part of the Planisphere itself. These can be placed over the basic maps, leaving visible only those constellations and stars seen from the chosen latitude. There are four masks, for latitudes 20, 40, 60,

80 degrees north. Stars for latitudes in between those given can be easily estimated.

Much valuable information is given in tables and lists on the back of the maps. Included are a section on orientation, planet positions through 1952, and constellation and star names and pronunciations.

It is safe to say that while many fine planispheres have appeared on the market, this is one of the most adaptable and usable for the navigator. Navigators learning unfamiliar stars far from home will find it easy with this Planisphere to orient themselves to new skies.

Stars Over Manhattan

Late—after midnight—thrilled by autumn air,
And standing at a height man's fancy built,
I look upon this fascinating world,
That never sleeps, and never is at rest,
With perfect pleasure. Through the streets below,
As through a system of illumined veins,
Swift-flowing life is moving back and forth;
Innumerable windows flash and blink
Light greetings through the cool, beloved space,
While—crowning all—a curtain made of stars
Is spread above the island of Manhattan.

Some hundred years ago, an Indian tribe
Was living here amidst the wilderness.
Its venerated chief (his name unknown)
Used here to sit upon a granite rock
And, with his head in silence lifted high,
Admonish all the members of his clan
To watch the wondrous, ever-circling orbs
—Those sparks in a tremendous canopy,
Those brilliant diamonds beyond their reach—
Which under other names than that of stars
Stood far above the island of Manhattan.

Of all the millions that have lived here since,
How few have known the chief's imposing figure.
How few, in pure amazement, silently,
Have gazed at Cassiopeia, Hercules,
Andromeda, Orion, and the Dragon.
Those tiny lamps which guard the universe
Were lit before our earthly days began.
Lift then your eyes—my friends—and see them glow!
And think that still, when all of us have passed,
The airy curtain of autumnal stars
Will stretch above the island of Manhattan.

—GERHARD FRIEDRICH.